

Contemporary Psychology

A JOURNAL OF REVIEWS

February 1961
VOLUME VI • NUMBER 2

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Published by THE AMERICAN PSYCHOLOGICAL ASSOCIATION

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Communications concerning subscriptions, change of address, claims for the nonreceipt of a number, advertising, and other business matters should be sent to the American Psychological Association, Inc., 1333 Sixteenth Street, N.W., Washington 6, D. C. Address changes must reach the Subscription Office by the 10th of the month to take effect the following month.

Contemporary Psychology: A Journal of Reviews is published monthly. The yearly volume comprises approximately 384 pages. The subscription per year is \$10.00, foreign \$10.50, single number \$1.00.

Published by the American Psychological Association at Prince and Lemon Sts., Lancaster, Pa. and 1333 Sixteenth St., N.W., Washington 6, D. C.

Second-class postage paid at Lancaster, Pa., and at additional mailing offices.

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Contemporary Psychology

A JOURNAL OF REVIEWS

VOLUME VI

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Hope Comes to Learning Theory

O. Hobart Mowrer

Learning Theory and Behavior. New York: John Wiley & Sons, 1960.
Pp. xiv + 555. \$6.95.

Reviewed by ABRAM AMSEL

Dr. Mowrer, president of the American Psychological Association in 1954, is almost too well known to need introduction. He is Research Professor of Psychology at the University of Illinois. He has written Learning Theory and Personality Dynamics (1950) and Psychotherapy: Theory and Research (1953), and has now after long gestation (yes, that's the right word; look it up) become the father of twins, this book and its co-twin that will be coming along shortly. He reviewed Woodworth's Dynamics of Behavior (CP, May 1959, 4, 129-133) when this book was an embryo. The reviewer is Dr. Amsel, newly come to the University of Toronto after a dozen years at Tulane University. Before that he was one of Spence's PhDs at Iowa. He has always been interested in the motivational variables in learning theory—at first in emotional drives, like fear, pain, and emotionality, in relation to hunger and thirst.

THIS new book of Mowrer's brings to mind Pandora's box. According to one version of the myth, when man, out of curiosity, opened this box, all of the blessings of the gods contained therein flew forth and were lost, and only hope, which was at the bottom, remained. The psychologist who is concerned with learning on opening Mow-

rer's book may find he has lost the 'blessings' of an uncritical attitude toward current learning theory and complacency about it, if he ever had them, not to mention the 'blessings' of some comfortably fixed generalities; nevertheless he will find that Mowrer has left him not only *hope*, but also *relief*, to counteract *disappointment* and *fear*.

This review covers the first of two volumes comprising Mowrer's major work. The second, *Learning Theory and the Symbolic Processes*, is advertised as providing, along with the present volume, "an orderly and powerful framework in which to conceptualize learning in general." In this first volume, which presumably provides the theoretical substructure for the second, Mowrer has with scholarly care reviewed a large segment of the recent literature on learning. (Out of about 1,000 references, over 700 date between 1950 and 1960 and 850 between 1940 and 1960; and the footnotes clearly indicate that Mowrer was revising and adding more recent references until the last pre-publication minute.) Taking a historical view of the field, he discards Pavlovian and Thorndikian thinking as oversimplified in their desire to "base [a true science of behavior] upon observable correlations between stimulation and overt response" (p. 47). He holds that "strict behavior-

ism is not acceptable . . . and has to be modified, at the very least, by the admission of the concept of emotion" (p. 62). He asserts in several places—without justification, I think—that one of the great shortcomings of Hullian theory has been its reluctance to handle the concept of fear (e.g., p. 138). "In the end," he says, "we are going to discover that all learning is reducible to conditioning" (p. 62). He proceeds to develop an explanatory scheme, based entirely on the classical conditioning of implicit ('emotional') responses, which makes unifying psychological concepts of such everyday words as *hope*, *relief*, and *disappointment* and gives meaning to them in learning theory—these in addition to *fear*, which has had a place in the literature of learning theory for some time.

In all of this there is further evidence that Mowrer, working at the business of theory and not of application, is still willing, almost eager, to translate the principles of learning theory to a language of everyday usage. This is not something new in the book. It has been more or less a characteristic of his writings in learning ever since his earliest great contributions. The esoteric cloak which dresses the language of learning theory is for Mowrer something to be torn off, baring the naked and basic meaning which must be communicated. There are those, including this reviewer, who may feel that while some ideas can be more *widely* communicated when the clothing of more or less exact technical language is removed, the consequences may be not unlike those attending nonmetaphoric removal of clothing: the number of relatively exact ideas communicated un-

der these circumstances may be somewhat restricted.

INTERESTS in current learning theory can be divided into two classes: those which deal with the acquisition of knowledges (information), skills, and games ('human learning'), and those which have to do with more general, motive-incentive oriented (instrumental) learning. The former class involves instructions, induced sets, and verbal mediation; the latter, a kind of learning which, in addition to being more general (observable in a variety of organisms), is usually 'nonintentional' and nonarticulate. Theorists, like Hull and Spence, most often criticized as 'mechanistic,' have been dealing almost exclusively with goal-oriented behavior of nonarticulate organisms and with the behavior of humans operating at pre-articulate and subarticulate levels. In man, as well as in animals, this kind of learning involves mainly conditioning and its derivatives, stimulus-selective learning and response-selective learning, determining the strengthening and weakening of approach and avoidance tendencies, broadly conceived. When one considers such learning, the topography of the criterion experimental response is usually of very little importance per se. It is at this level of learning that hypotheses derived from animal studies have been applied most profitably to the understanding of man's adjustment to his environment (i.e., to the development of 'personality'). If you doubt this assertion, ponder for a moment on where in the *learning* literature you would go to find hypotheses applicable to the explanation of a child's tantrums, or an adolescent's conflicts, or an adult's alcoholism.

In the book under consideration, Mowrer addresses himself to what might be called 'personality-learning'; and, despite substantial differences in conceptualization, those things which characterize the interests in personality-learning of theorists like Tolman and Hull and their descendants characterize this book of Mowrer's also. One of the most evident characteristics, although not the most important, is the use of animals, particularly rats, as subjects in experiments. As a conservative estimate, four-

fifths of the references in this book are to rat experiments, or else to nonexperimental articles based primarily on rat experiments. Does it seem paradoxical that theorists in 'personality-learning' should look to rat research for their experimental bills of particulars? It may seem so, but learning theorists, oriented toward personality, as is Mowrer in this book and as is Spence in his *Behavior Theory and Conditioning*, are somewhat restricted to this choice of subjects: rats or comparable animals; very young (pre-articulate) 'humans,' sometimes called children; or older humans operating in 'artificial' experimental situations from which the advantages gained by symbolic activity have been removed (e.g., eyelid or GSR conditioning).

The fact that Mowrer, who certainly cannot be cast as a black, S-R, rat psychologist, and who calls repeatedly for "liberating" learning theory from the "bondage" of the S-R habit formulation and reflexivity (e.g., pp. 221, 251), still must seek rat experimentation (a good part of it from the most mechanistic quarters) for empirical support should be instructive to some psychologists. It means that these "mechanistic" (a word which Mowrer also uses) rat psychologists have been working in areas of great importance to a "non-mechanistic," personality-oriented, learning theorist who, in this book at least, apparently finds little use for human learning experiments.

As I view it, the kind of systematic approach purveyed in this book has more the flavor of an older than a newer theoretical *Zeitgeist*. The relatively broad systematic coverage of behavior, outlined by such learning theorists of the last three decades as Guthrie and Tolman and, to a lesser extent, Hull, was a carry-over from attitudes—not completely worn off—acquired from contact with early philosopher-psychologist systematists like Titchener and William James. The systematist had to have something to say about everything—a philosophy of psychology, almost. We seem now to be in a different theoretical atmosphere in psychology, one more appropriate to our stage of development. This is an atmosphere in which theory is made in smaller pieces than

before and is quickly reduced to experimental operations. It is an era of tighter, more data-bound generalizations which attempt to organize separate problem areas and to gain reasonable predictive control over them with less self-consciousness and much less concern about theoretical unity and breadth. You get this feeling of changed emphasis when you read the current Estes, or Neal Miller, or Spence, and Frank Logan's *Incentive* (Yale Univ. Press, 1960) is a good recent example.

This trend toward fragmentation of 'systems' into theories dealing with parts of systems, if I read the times correctly, Mowrer's book does not follow. Here is, rather, the approach to theory of the systematist-scholar: an attempt to reduce a diversity of experimentally-derived 'facts' from quite different problem areas to a few explanatory principles, a deep and earnest concern with what 'the theory' may fail to encompass. Mowrer's own feelings in this matter are revealed (p. 248) when he says, "The success or failure of the present theory, in one important respect, will hinge upon how well it articulates with and unifies the view of others."

Now as to the theory itself. Mowrer indicates it is two-factored, not in the sense of his older distinction between autonomic fear conditioning and instrumental habit formation, but in the sense of "two basic, and basically different reinforcement processes," which he terms decremental or type-D (reward) and incremental or type-I (punishment). This is a movement away from the later Thorndike and Hull on the conception of negative reinforcement. These two reinforcement processes are the UCSs for the conditioning of *hope* and *fear*, respectively, which nevertheless may be conditioned on the basis of contiguity alone. In fact, *only* emotional responses (hope, fear, and their antitheses) are conditionable; and *only* conditioning is learning. Overt, skeletal-muscular responses are not learned but are merely selected on the basis of these secondary-reinforcing emotions, and this for Mowrer is the phenomenon of "habit"—a habit concept which, without the associative connotations familiar to Hullians and others, is restricted to instrumental behavior. (Conditioned responses are not habits.) Apparently, Mowrer does not regard as learning changes in response evocation probabilities, but only the

strengthening of implicit emotional responses underlying these probabilities.

How does a simple habit (in Mowrer's sense) get established? Here is an example: A hungry rat runs down an alley, finds food, and eats. The finding of food assures that the stimuli related to running will be conditioned to hope, so that, subsequently, the same rat running in the same alley will experience hope which will secondarily reinforce running. The "habit" of running, according to Mowrer, is the secondary-reinforcing emotion of hope, which is conditioned to response-related stimulation from running. Therefore, all learning reduces to conditioning of the appropriate emotion—*hope* in the case of decremental reinforcement, *fear* in the case of incremental reinforcement—because hope (secondary reinforcement type-2) and fear-reduction (secondary reinforcement type-1) are the mechanisms of habit formation. But it is interesting that while Mowrer wants to get psychologists out of what he calls "S-R bondage," his own conception of habit substitutes "increased conductivity between the stimuli which response R_i [instrumental response] produces and the phenomenon of secondary reward, or 'hope.'" Since hope is itself a conditioned response, we would seem to have moved with Mowrer out of one "S-R bondage" and into another.

But even with the introduction of hope based on reward in the revised two-factor theory, fear is still the central concept for Mowrer. All primary motivation has a component of fear. Following Neal Miller, fear is the conditionable component of primary pain motivation, and this is extended in the form of hunger-fear and thirst-fear to deprivational (appetitional) drives (pp. 145ff.). This conclusion leads Mowrer to such statements (p. 128) as "secondary reinforcement types 1 ['fear reduction'] and 2 ['hope'] both involve fear reduction." (There are those who would be happier if Mowrer's italicized portion read: *are both involved in fear reduction.*) The ubiquity of fear in Mowrer's system is also apparent in his interpretation of the frustration effect: "Thus it is almost axiomatic that 'frustration,' in the objective sense of that term, involves the nonfulfillment of an implied 'promise' and the emergence, at the very least, of the experience previously termed *disappointment*, which is to say the *loss of hope* and the *return of fear* and the resulting *increase in motivation* found by Amsel & Roussel" (p. 406). Ultimately, then, the frustration effect is attributed to a roundabout "return of fear." Hope, it seems, is the equivalent of 'not-fear' and 'not-hope' is the equivalent of fear.

I am not, at present, disposed to feeling too relaxed about a theory whose *sine qua non* is secondary reinforcement. There are two reasons for this state. First, I am not as convinced as Mowrer about what the secondary reinforcement (type-2) literature demonstrates. (Mowrer can be forgiven if, like all theorists, he includes most of the positive and little of the negative evidence.) Secondly, I have been impressed with the possibility that what Mowrer calls secondary reinforcement type-2 is confounded by frustration and its reduction. It occurred to me (AAAS Symposium, 1958), as it may have to others, that there is a "similarity of operations which define [Mowrer's type-2] secondary reinforcement effects (presence of cues associated with reward when reward is absent) and frustra-



O. HOBART MOWRER

tive nonreward effects . . . raising the possibility that at least some of the effects which we have been attributing to secondary reinforcement may actually depend upon the arousal of frustration and its reduction." The Zimmerman method for producing durable secondary reinforcement (*Psychol. Rev.*, 1957, 64, 373-383), on which Mowrer relies so heavily, seems to me particularly susceptible to such an interpretation.

On top of these hesitations about secondary reinforcement type-2, there is reason for being uncomfortable about secondary reinforcement type-1. This is, perhaps, more a matter of terminology than anything else: The distinction between 'primary' and 'secondary' as applied to reinforcements has, at least in terms of a most common usage criterion, been a distinction between unlearned and learned. Mowrer has for some time referred to fear reduc-

tion as a form of secondary reinforcement (now called type-1), but it has always seemed to me that fear reduction can be either primary or secondary reinforcement in the unlearned-learned sense. If a signal (CS) comes to elicit fear as a CR, the removal of the signal will by definition eliminate the CR and reduce the fear. The organism need not learn not to be fearful in the absence of the CS. The 'relief' that accompanies the removal of a danger signal is, in this sense, a primary reinforcement. On the other hand, a safety signal is a stimulus which consistently accompanies fear reduction and is a learned reinforcement like any other (type-2) secondary reinforcement. In the sense of the usual (learned-unlearned) distinction, then, the terms of Mowrer's schema (p. 213)—"danger signal off (relief)" and "safety signal on (hope)"—can not be equivalent forms of secondary reinforcement as his analysis seems to require. Further, Mowrer's dual conception of secondary reinforcement seems to blur the distinction between secondary motivation and secondary reinforcement, which the older single conception of secondary reinforcement preserves, without providing any compensating theoretical advantages which are readily apparent.

FINALLY, some general impressions. The book is written with a strong historical emphasis, and a great amount of its space is devoted to detailed descriptions of the work of others. (There are perhaps more and longer direct quotes here than in any book I have ever seen.) Partly because of the discontinuity which this creates, the theory itself unfolds like the plot of a 'whodunit'—there is suspense as Mowrer lets the reader look through a haze, seeing faint outlines at first, until finally the pattern of theory emerges. The numerous and extensive quotations, always a characteristic of Mowrer's writing, are very instructive; they reacquaint the reader with many important portions of the psychological literature, yet they do distract him and frequently they reduce the effectiveness of Mowrer's argument.

If one overlooks these long interruptions by the quotations, Mowrer's book, in outline, has something of the characteristics of a teaching machine, programmed for teaching a theory. The reader (learner) is brought along in small steps; the same principle is stated in a variety of ways on different pages; and what Mowrer is driving at begins to

come clear after five or six chapters. Chapter 7 then gives a rather direct statement of the theory. While this approach may, as Mowrer himself claims in the preface, be useful when the book is used as a text, I have a feeling that his peers, who should and will study this book for its contributions to learning theory—this kind of book is not ordinarily used as a text in the usual sense—may wish he had made a more concise statement of his position, with fewer long excursions and detours off the main path of his thinking. The scholarly documentation of arguments with experimental data is too often diluted by anecdotal 'evidence' which is usually contained in footnotes that are

too long and, when read, reduce the set of the reader too much.

Mowrer's book will be influential—there is much here for the social and clinical psychologist as well as for the learning theorist to chew on and try to digest. Whether or not it will be important in the long view will, of course, depend upon its use, not only in classrooms but also in laboratories. I would predict that many a graduate seminar will be dissecting Mowrer's book in the next few years, and profitably so. If this experience creates the kind of excitement which inspires significant scientific investigation, the book can be called important. Predictions in this realm are so dangerous to make.

a studious *Introduction* to the whole. The long introductory chapter by Henry A. Murray, *Historical Trends in Personality Research*, does a pretty effective, if whimsical and speculative, job of relating trends in personality theory to trends in personality research, and showing how the various papers in the volume adumbrate these trends, although it must be admitted that Murray has to stretch a point here and there to attach any real historical significance to some of the papers reproduced here.

THE first group of papers is headed *Explorations in Behavior* and deals principally with experimental studies of psychodynamics. These include Daniel R. Miller's studies of denial mechanisms, Irving Alexander's and Arthur Adlerstein's studies in attitudes toward death, Noel Mailloux and Leonardo Ancona's studies in the psychopathology of religious attitudes, Gerald Blum's conceptual framework for a psychoanalytic behavior theory (in which the Blacky pictures provide stimulus input for the personality conceived of as a closed-circuit television system), and A. B. Luria's studies of voluntary action in children, based on a Pavlovian electro-mechanical system, as befits a Russian psychologist. In his commentary on this group of papers, Silvan Tomkins points out the pervasive Freudian influence on psychodynamics is represented by all of the contributions save the Russian. But Freud or no, there is another common aspiration evident in the approaches of Blum and Luria. As Tomkins aptly points out, "Here then are a Pavlovian and a Freudian-electronic comrade, cybernetic bedfellows." Neither Tomkins nor Murray seems bored, as this reviewer is, by the persistent international pursuit of psychophysical models for the understanding of human behavior, as though psychology can never rest easy among the sciences until modern electronics has made good the failures of an earlier oversimplified psychophysics.

The next group of papers comes under the heading of *Person Perception*. In his commentary on this topic, Robert Macleod, like Murray, displays commendable erudition in considering whether person perception may be properly in-

Personality Research Goes International

Henry P. David and J. C. Brengelmann (Eds.)

Perspectives in Personality Research. New York (44 E. 23rd St.): Springer Publishing Company, 1960. Pp. x + 370. \$7.50.

Reviewed by G. M. GILBERT

The editors are both deeply interested in the psychology of personality and its assessment. David, who is Chief Psychologist for the New Jersey Department of Institutions and Agencies, has long been especially concerned with promoting international relations in psychology, and Brengelmann, with a Göttingen MD and a Maudsley-London PhD, now working with Eysenck at Maudsley, provides internationalism for what has been an American-dominated field. The reviewer, Dr. Gilbert, Professor of Psychology at Long Island University and chairman of its department of psychology, is right now President of the Interamerican Society of Psychology. The Society for the Psychological Study of Social Issues gave him their award for his study, *The Psychology of Dictatorship*.

THIS volume is an edited selection of papers from the Third International Congress of Psychology in Brussels in 1957. It must therefore be appraised with due regard to the limitations of a collection of papers read at

an international congress, rather than as a book written for the express purpose of doing justice to the subject matter indicated by its title. These limitations provide many sources of variability and incongruity, such as the vastly different stages of development of psychology in different countries, semantic as well as *weltanschauliche* differentials in conceptualization; cultural differences in disciplinary distinctions and academic courtesies which often determine participation in such congresses, and finally the purely fortuitous circumstances determining who is able to finesse a travel grant or take a tax-exempt vacation to a particular congress at a particular time.

This comment on the book's origin is not intended to prejudge the undertaking, but rather to underline the achievement of the editors in producing a volume of such relatively high caliber and degree of integration. The integration is managed largely through the device of selecting and organizing papers under three topical perspectives, with a specially written commentary on each, and

cluded within the domain of processes traditionally subsumed under *perception*. Macleod says *yes*, but Murray says *no*. I would venture the opinion that both learned gentlemen are wasting their scholarship on a semantic quibble, since the individual's ego-involved awareness of other people is a manifestly significant area of personality research, regardless of the term used to signify awareness.

The final section of the book combines a number of papers under the heading of *Resources*, which might more appropriately have been labeled *Miscellaneous*. These include some longitudinal studies in child development, a paper on suicide, and three papers on different aspects of assessment.

Taken as a whole, the Brussels volume will be disappointing to those who look for broadly representative pioneering studies in personality and for striking novelties of approach emerging from different parts of the world. The APA-meeting habitué will find little here that is particularly novel; indeed, he may be impressed by the predominantly American participation and flavor of the whole volume. Yet this is not the fault of the editors, but a simple commentary on the fact that American psychology, with its manifest European heritage, dominates the discipline of psychology in the world today and a congress held in Brussels will inevitably reflect that dominance without Latin American or Asiatic representation. One deficiency that is conspicuous, however, even within the usual limitations of a volume so conceived and so dedicated, is the lack of examples of cross-cultural research. Here indeed is a vital new perspective in personality research which has been notably absent from APA programs, dominated as they are by PhD theses cut to fifteen-minute presentations, but increasingly represented at meetings of the Interamerican Society of Psychology and in journals like the *International Journal of Social Psychiatry*.

The reader who would like a well-edited selection of some of the more interesting papers at the Brussels meeting will find this volume well done, but he who seeks a textbook on contemporary frontiers in personality research will not be rewarded here.

Persons, Places, and Psychoses

H. Warren Dunham

Sociological Theory and Mental Disorder. Detroit: Wayne State University Press, 1959. Pp. xii + 298. \$5.50.

Reviewed by DANIEL R. MILLER

who is Professor of Psychology at the University of Michigan and a Research Associate in its Institute of Social Research. His recent book with G. E. Swanson, *Inner Conflict and Defense* (Holt, 1960), takes account of Dunham's ideas on the ecology of psychosis. Just now Miller is doing a chapter on personal identity for Koch's fifth volume.

IMPRESSIVE evidence of the hereditary and physiological causes of psychosis have earned the increasing attention of psychologists, who place much less weight on environmental stress than they did in the heyday of Watsonian behaviorism. The relative emphases on nature and nurture, soma and psyche, are bound to change with the recent proliferation of publications on the ecology of personality disorders. In this volume, Dunham, one of the pioneers in the field, has summarized some of its goals, considered the strengths and weaknesses of its methods, and reported some of the more challenging findings. Most of these observations have been included in reports of his own research in which he related the incidence of psychoses to indices of poverty and disorganization of the family, old age, and the dislocations caused by war.

Of the fifteen chapters, twelve were adapted from papers published over the past twenty years. Dunham has created a volume with considerable organic unity by summarizing his purposes in the introduction, revising the papers slightly to point up their interrelationships, and adding three previously unpublished studies.

The ecologist makes a number of de-

batable assumptions when he makes inferences about the regional distributions of psychoses from data he has collected from hospitalized patients, or when he makes deductions about the causes of a psychosis from its frequencies in different neighborhoods. In a dispassionate and sophisticated manner, Dunham discusses alternative evidence for premises like the following: patients with different disorders or from different neighborhoods have equal access to hospitals; most psychotics eventually get hospitalized; discrepancies in systems of diagnosis have little effect on the proportions of reported psychoses; the high frequencies of some psychoses in neighborhoods with low average incomes mean that poverty contributes to the incidence of these psychoses, not that psychotics lose their jobs and drift to such neighborhoods because of the low rents.

DUNHAM's many theoretical speculations are usually phrased in the form of testable hypotheses and reflect a shrewd appreciation of critical theoretical issues. His hypotheses bear on a host of problems the range of which can only be suggested by a few examples: the reasons why income is significantly related to the incidence of schizophrenia but not of manic-depressive psychosis; the extent to which psychosis can be explained by cultural factors, latent traits of patients, and interactions between the two; the manner in which the shyness and suspiciousness of the incipient psychotic contribute to breakdowns by alienating potential friends and reinforcing the tendencies toward social isolation and the misinterpretation of social events.

The reader is bound to be critical of some aspects of papers that were published over a period of twenty years and that portray the halting steps of an investigator in a relatively new discipline. Three weaknesses stand out. First, control groups were not used in studies of the pre-morbid personalities of catatonic and paranoid schizophrenics. Secondly, in the consideration of pre-morbid personality the writer is unconcerned with psychological dynamics, which provide alternative though not incompatible ex-

planations of etiology. Finally, one chapter, which is devoted to psychoanalysis as an ideology, does not seem relevant to the rest of the book and portrays a position that is confusingly ambivalent. One gets the impression, however, that debatable positions and even some missteps are inevitable in the work of a

man who has not hesitated to speculate imaginatively about many new problems. Readers of this book will be impressed by the clarity of Dunham's thinking, the importance of his field, the fruitfulness of his speculations, and some of the significant investigations to which they have led.

Fried and Freud

Edrita Fried

The Ego in Love and Sexuality. New York: Grune & Stratton, 1960. Pp. viii + 296. \$5.50.

Reviewed by ALBERT ELLIS

The author, Dr. Fried, is Senior Supervisor in the Postgraduate Center for Psychotherapy in New York City. She holds a PhD from Vienna and has filled various important psychiatric posts in New York City during the last twenty years. She is especially interested in group psychotherapy and in studying the artistic and creative processes. The reviewer, Dr. Ellis, is a well-known psychotherapist and marriage counselor in New York City who has published extensively on sex, love, marriage, and family relations. His latest book is The Art and Science of Love (Lyle Stuart, 1960). Next February Hawthorn Books will publish under his editorship an Encyclopedia of Sexual Behavior, two volumes containing articles by sexologists, psychologists, sociologists, anthropologists, and other scientists.

IT is high time that someone wrote a reasonably definitive book about the enormous significance of the ego, self, or (much better) human person in love and sexuality. And Dr. Edrita Fried, with her Viennese PhD, her special training in communication research and applied psychology with Lazarsfeld and Cantril, her solid background in individual and group psychotherapy, and her directorship of a project to study the development of creativity in artists undergoing therapy, should have been the ideal person for the job; and *The Ego in Love and Sexuality* should have been one hell of a good book. But it is not.

What went wrong? A clearcut clue is given by Dr. Fried in the preface to her book, where she credits Miguel Prados, Richard Sterba, Margaret Mahler, and the late Geza Roheim as the strongest influences on her thinking. Obviously, from her own statement and from the parochial character of her references throughout the book, her main supports for most of her views come from fairly orthodox psychoanalytic writers. If Dr. Fried had been as dependent, say, on some orthodox ecclesiastical authorities as she is on the highly select psychoanalytic confraternity which she continually cites, her book could hardly have been more one-sided.

More concretely: *The Ego in Love and Sexuality* rarely discusses such "normal" sex-love questions as how a person's estimation of himself is significantly related (a) to the intensity, frequency, and duration of his sex outlets, (b) to the various kinds of sex behavior in which he engages, (c) to his tendency to become infatuated with or to love others, or (d) to his actual success in working out sex-love relationships. Instead, the book is replete with case histories of individuals who, when contemplating orgasm, experience extreme floating and flying sensations, anxiety and revulsion, violent hatred of their partners, oral or sexual apathy, and various other borderline psychotic manifestations. And in its consideration of love it is largely concerned with narcissistic isolation, clinging between mates, the compulsion to repeat love situations,

and the pains of boredom in love relationships.

If my patients (many of whom come for treatment of specific sex and love problems) were to display a tithe of the severe symptoms which Dr. Fried describes in one case illustration after another, I would seriously question their sanity. Yet she seems to believe that all of us, when we really get down to the deep, deep layers of our sexual-amative being, are similarly afflicted. A striking example, her chapter on masturbation, ignores the fact that probably 98 percent of autoerotism has little or no pathological significance and catalogues several masturbatory aberrations that are rarely, except by dedicated psychoanalysts, found even in emotionally disturbed persons.

Some of the other perennial Freudian-oriented hypotheses which are accepted as true are: "Sexual passion is determined . . . by the individual's capacity for enduring and enjoying regressive processes" (p. 17); "clitoral orgasms require lighter and briefer ego changes than the vaginal orgasm" (p. 19); "if each partner obtains sexual climax alone, the excitation is definitely cut down" (p. 64); "prolonged and repeated masturbation alienates the person from . . . challenging and satisfying contacts with others" (p. 156); and promiscuity almost always stems from psychopathic behavior and "is a desperate defense against loneliness and apathy" (p. 260). Even a cursory examination of the works of Havelock Ellis, Alfred C. Kinsey, G. V. Hamilton, G. Lombard Kelly, Norman Haire, W. H. Masters, and dozens of other leading authorities on sex and love, whose names are conspicuously absent in this book, would throw serious doubt on the validity of these assumptions.

As is true of scores of other psychoanalytic books of this genre (or, one is tempted to say, of the single book that Grune & Stratton and International Universities Press keep republishing under different names), *The Ego in Love and Sexuality* is chockful of obfuscating jargon, of 'proof' by cultish quoting, and of continual reification of abstract terms. Witness a typical paragraph:

"The defective ego cannot cope with

the ego regression or ego reduction that occurs at the onset of warm, affectionate and sexual feelings and reaches its high point in orgasm. *The defective ego functions on the 'all or none' principle.* It cannot readily make minor or major transitions from solid to fluid structure which specific situations demand. It cannot rally from partial ego regression to a quick ego consolidation. Its capacity for undertaking changes in ego structure, for the discriminate discarding of certain ego defenses and functions and the gradual or quick selection and marshalling of ego functions and defenses, is low. It is either in a state of armed preparedness or in a state of inactivity. The latter condition is experienced as threatening because quick and selective transitions to partial activity are difficult. *The inability to proceed from partial or total regression to suitable degrees of ego vigilance and ego activity is an ego deficit which obstructs the enjoyment and endurance of ego dissolution in love and sexuality*" (pp. 34f., italics Dr. Fried's).

This is not to say that Dr. Fried's book is a total loss. It isn't. When she occasionally becomes nonpsychoanalytic she says sane and intelligent things. She favors, for example, highly active psychotherapeutic intervention, thinks that homosexuals are definitely treatable, recognizes that human beings require considerable sexual change as well as amative steadiness, and actually holds that the Oedipus complex business is overrated. One gets the impression that, if she buried Prados, Sterba, Mahler, Roheim et al. (and perhaps resurrected Lazarsfeld and Cantril), she might write a valuable book in this field.

For the nonce, however, instead of the major work on the human person in love and sexuality which could have been (and still should be) written, *The Ego in Love and Sexuality* presents us with a book that more accurately might be titled: *A Fairly Orthodox Psychoanalytic View of Some of the Most Pathological, Minor, and Unimportant Aspects of Love and Sexuality Found in a Sample of Exceptionally Aberrated Individuals Selectively Attended to by the Author and Various Other Members of Her Cult.*

Clinics for Child and Parents

Alan O. Ross

The Practice of Clinical Child Psychology. New York: Grune & Stratton, 1959. Pp. x + 275. \$5.75.

Reviewed by ROBERT D. WIRT

The author, Dr. Ross, is Chief Psychologist of the Pittsburgh Child Guidance Center and also adjunct Associate Professor at the University of Pittsburgh. Since his Yale PhD in 1953 he has been mostly concerned with parent-child relations and the effects of brain injury in children. See the review for more about him. The reviewer, Dr. Wirt, a Stanford PhD of 1952 who acknowledges the influence of Maud A. Merrill, is Associate Professor of Psychology, Child Development, and Psychiatry at the University of Minnesota, Coordinator there of the Clinical Child Psychology Training Program and newly appointed Director of the Center for Personality Research. His main concern has been with children's anxieties and delinquency, but just now he is concentrating on getting the personality center under way.

IN the past decade there has been throughout the nation an enormous growth in the number of community mental-health centers. Most of these centers are supported by public subscription and tax funds. Their major business is work with disturbed families. They constitute one cause for the expanding job market for clinical psychologists skilled in diagnosis and treatment of emotionally disturbed children and their parents. Although various committees of the APA have concerned themselves with the problem, thus far no one in professional psychology has suggested a widely acceptable solution to the manpower problem created by these rapidly increasing facilities. Alan Ross is an articulate proponent of a point of view which recognizes the emergence of a new professional specialty:

clinical child psychology. Most readers of *CP* will be pleased that Ross follows the party line of the APA fairly closely regarding such important professional issues as relations with psychiatry, standards and level of training and experience, and areas of competence for psychologists. These facts should be good for student readers of his text.

No one who reads his book is likely to doubt that Dr. Ross himself is a wise and gifted psychologist. After ten years of experience in applying clinical methods to work with children and their families, he has written a text in which he hopes to integrate areas of clinical and developmental psychology which are fundamental to what he sees as the practice of the new specialty in clinical child psychology. Readers will find Ross' alloy contains a very large portion of clinical psychology with a dash of developmental. There is little here which demonstrates much contribution to clinical practice from traditional child psychology, but there is much which elucidates the need for special training and experience to equip the clinical psychologist for engaging in diagnosis, treatment, and research with disturbed family relationships.

THE nearly exclusive focus of the author's comments and examples is on the clientele of community child-guidance clinics. He says he chose this perspective to give the account an authenticity which would be lacking if he wrote about aspects of practice with which he has only vicarious familiarity. He succeeds. The great strength of this fine book is the success with which Ross describes in clear language many aspects of the psychologist's role as he functions in a community clinic. Because of the author's emphasis, however, the text will need to be augmented by other material if students are to learn about the wide range of activities engaged in by clinical child psychologists in schools, hospitals, courts, and residential treatment centers.

Ross has dramatized his discussions with excellent case examples which are appropriate, well described, and in sufficient number and variety. If, however, one is looking for an intensive and detailed analysis of either diagnostic or

therapeutic practice in work with children, he will not find it in this book despite its title. With the exception of the chapters on cerebral functioning—brilliant chapters—the book suffers from lack of detail.

In describing the areas of activity in the child guidance clinic, Ross gives his characterization of an "ideal case." He states that "anyone familiar with child guidance treatment will recognize that few cases ever approximate this postulated ideal" (p. 26). He defends its inclusion on the ground that the criteria presented may stimulate research. It seems to the reviewer that a beginning textbook is an unlikely place to stimulate research in psychotherapy. The criteria for selecting the ideal case (which in sum describes a mildly disturbed, intact, and highly motivated family from the same cultural milieu as the therapists, who are predicted to need short-term, ego-supportive treatment) seem to imply a recommendation that other cases not be treated by community clinics. If not to the community clinic, where, then, are other community agencies to refer? Many cases, moreover, which fit the author's "ideal," are and can be treated by the school psychologist, social worker, or counselor, and by the family physician. In fact, frequently community clinics are asked to suggest solutions for families with whom other agencies have failed.

The first part of the book, which describes the roles of the clinical child psychologist, is suitable for beginning graduate students. It is a comprehensive, though not detailed, description of functions of the clinical child psychologist as preformed in an eclectic, contemporary child-guidance clinic with an orthopsychiatric orientation. The second part of the book is devoted to the instruments used by clinical child psychologists in evaluating children. It is an excellent summary which will be helpful for students who have had some coursework in psychometrics and projective techniques. In the reviewer's opinion the entire book would be improved as a textbook if both sections were substantially expanded without injuring the present high level of communication.

Soziale Perzeption

Gerhard Kaminski

Das Bild vom Anderen. Berlin: Georg Lüttke Verlag, 1959. Pp. 232.

Reviewed by MICHAEL WERTHEIMER

who is Associate Professor of Psychology at the University of Colorado. He has studied at Swarthmore, Hopkins, and Harvard, taught at Harvard, Hopkins, and Wesleyan before he went to Colorado. He is deeply concerned with perception and the so-called higher processes. With D. C. Beardslee he published Readings in Perception (Van Nostrand, 1958; CP, Feb. 1959, 4, 42f.) and he edited his father's, Max Wertheimer's, enlarged edition of Productive Thinking (Harper, 1959). Just now he is busy with research on what he likes to call "person cognition," studies of the ways in which people describe other people. On this business he has two reports already in press.

To a US-trained psychologist, this is an exasperating, impressive, frustrating, fascinating work in the thorough German tradition. Rarely has a more scholarly aperitif been concocted; it titillates the appetite, whets curiosity, provides a fresh, enticing aroma. No wonder this dissertation earned its author his PhD *summa cum laude*.

Discursive, but frequently incisive, this thesis reviews much of the English and German literature on forming impressions of persons but reviews it creatively and thoughtfully rather than by sheer cataloguing. Ideas common to various studies are clearly abstracted, and their theoretical bases, both epistemological and methodological, are illuminatingly explored, often at length.

One gets the impression that Kaminski has implicitly tried to do for the broader philosophical and theoretical issue of impression formation (with a detailed ancillary examination of clinical diagnosis, and with an attempt to place all this within the framework of psychology in general) what Cronbach did for the methodology of studies of the accuracy of interpersonal percep-

tion. The difference between Kaminski's and Cronbach's approaches, one is tempted to speculate, is largely a reflection of the difference between the European and the American *Ortgeist*. Cronbach emphasizes methodological and mathematical sophistication and is somewhat impatient with procedural errors or operationally untouchable or naive questions; methodological defensibility, even purity, is his aim. Kaminski stresses dialectics, reasoning to the core of an issue, is willing to examine problems which he considers by their very nature inaccessible to observation, and is disdainful of premature empirical work; his aim is to get to the heart of a question.

AFTER a lengthy introductory discussion of the philosophical bases for his investigation, Kaminski proceeds to the main portion of his thesis, a section which he entitles, *Psychological Diagnosis as a Social Perception Process*, but which seems more concerned with social perception as such than with diagnosis.

His semi-systematic survey begins by considering a series of 31 unsorted concrete instances of real-life social perception, such as: a customer comes into a clothing store, looks around, expresses wishes, lets herself be served, chooses, etc.; the saleslady gradually 'attunes' herself to her client. Or: daily one goes to work in the same conveyance as another person, without ever having talked to him; one knows exactly how he opens his wallet, when he will offer his seat, how he blows his nose, etc. Or: one wants to give somebody (friend, spouse, child, etc.) a gift, and must consider the sort of person he is, to decide what present he would enjoy.

The discussion is based on an original framework too complex to present in a brief review. It involves such concepts as spatial or temporal extension (*Erstreckung*), which leads to such research questions as the influence of length of acquaintance or variety of observational situations on the formation of impressions of people, and whether a characteristic attributed to someone by an observer is considered a transitory manifestation or an enduring personality trait. Kaminski displays a thor-

ough acquaintance with Brunswik's plea for the importance of ecology, some prejudices concerning nomothesis and idiography, and makes full use of a very keen critical sense, functional bilingualism and sheer brilliance. It is a rich survey, bristling with fruitful insights and suggestions for empirical research.

He attempts to bring psychological diagnosis and person cognition together conceptually, concluding that most clinical techniques are really not very good at helping the psychologist obtain a thorough understanding of the individual case, and that these techniques violate many of the basic principles of efficient social perception.

It is (from the US vantagepoint) unfortunate that this work is in German, for it refers to a great deal of literature which seems unknown or unheeded in America. Whoever translates it into English will make a real contribution.

In brief, the discussion is highly competent and sometimes scintillating; many ingenious research suggestions emerge; but there the book ends. The typical American psychologist, if this reviewer be one, feels a lack of closure. Here are the prolegomena (he says to himself); now let us collect some data, check out some of these hypotheses, and follow up some of these leads. Hardly a number adorns this book. Major opus though this be, it does remain an *hors d'oeuvre*.

Kaminski is, however, unwilling to spend his entire professional life in the armchair. He recognizes that his volume presents a background and a program for research rather than a report of a completed empirical project. Correspondence between the reviewer and Professor Dr. K. S. Sodhi, Director of the Free University of Berlin's Institute for Psychological Research—where Kaminski is now a research associate—revealed that Kaminski is currently engaged in "the experimental verification of the views he discussed only theoretically in his book." This reviewer is one who will be looking forward eagerly to reports of Kaminski's further work.



Man is as full of potentiality as he is of impotence.

—GEORGE SANTAYANA

Parents Learn of Child Development

Louise M. Langford

Guidance of the Young Child.

New York: John Wiley & Sons,
1960. Pp. x + 349. \$6.25.

Reviewed by HAROLD ABEL

The author, Mrs. Langford, is Assistant Professor in the Department of Family and Child Development in Kansas State University's School of Home Economics. The reviewer, Dr. Abel, is Associate Professor at the University of Nebraska, Head of its Division of Family Relations and Child Development, and Director of its Child Development Laboratory. He was trained in child development at Syracuse University and has been at Nebraska since 1956.

FOR well over thirty years nursery schools have been maintained by colleges of home economics and education for the primary purpose of providing students a practical background in the guidance of young children. Typically, laboratory courses are conducted with concurrent lecture sections that cover the academic aspects of child development—the theory and the research findings. Many excellent textbooks admirably fill this role. Nevertheless, outside of the laboratory manuals, there is a paucity of information that is based on research findings and can be used with beginning students in practical situations with children. *Guidance of the Young Child* attempts to fill this need.

Louise Langford says that her book "offers specific and practical methods of guidance for the young child in group and individual activities." Thus her over-all purpose is essentially the same as Read's in *The Nursery School*, but Read's excellent work is aimed at a somewhat higher level than Langford's, which is intended primarily for beginning students in the child development laboratory. It is organized almost entirely around situations involving preschool-age children in group settings.

Because of its special orientation, and because it does not review research or present a comprehensive view of the field of child development, the usefulness of Langford's volume as a textbook is limited. On the other hand, a well-written book of this type can serve the needs of nonprofessional parents searching for realistic suggestions for the guidance of a child, a book devoid of specific references to research studies, a behavioral Dr. Spock for preschool-age children.

The number of topics considered is broad. In addition to the conventional areas of child development, the author has included sections on such diverse topics as food, sleep, clothing, creative media, and the handicapped child. Each topic receives a somewhat similar treatment: a brief description of the area; a few important general statements drawn from research, although almost never referred to as such; case study examples with evaluations; and, finally, practical suggestions for guidance, often in list form. The extensive use of well-placed and meaningfully descriptive pictures supplements the text.

Although there is a short list of supplementary readings at the end of each chapter, the lack of references to research is likely to dismay professional readers who are accustomed to seeing the evidence for conclusions along with the author's evaluation. The review of the principles of child development is, however, intentionally cursory and non-technically presented. This is not a textbook on child development in the conventional sense, and only major and well-accepted developmental concepts are included. Consequently, the beginning student and lay parent will find this book to be a readable, informative book.

The early chapters, and several isolated statements throughout the book, are concerned with the purposes and values of a university nursery school. Not every instructor in child development will agree with every statement, but the total impression is favorable. The reader receives a philosophy similar to that offered by Read which, in this reviewer's opinion, has been one of the soundest presentations available.

Psychologists, though they lay claim

to the study of human behavior, often disdain literary attempts concerned exclusively with the application of psychological research and theory; never-

theless the dissemination of knowledge creates a continuing need for books devoted to practical techniques. Such a book is *Guidance of the Young Child*.

Behind the Blue-Collared Apron

Lee Rainwater, Richard P. Coleman, and Gerald Handel

Workingman's Wife: Her Personality, World and Life Style. New York (80 Fourth Ave.): Oceana Publications, 1959. Pp. 238. \$7.50.

Reviewed by SIDNEY ROSEN

The reviewer, Dr. Rosen, is Associate Professor of Psychology at Marquette University. He has been a member of the Research Center for Group Dynamics at the University of Michigan and is now cooperating with Ronald Lippitt in research on social power in the family and among peers. He reviewed Stein and Cloward's *Reader in Social Science* (CP, Feb. 1959, 4, 50f.).

AT no small expense to Macfadden Publications (publishers of *True Story*, *True Experiences*, *True Romance*, etc.), this volume announces itself as "one of the most fully documented motivation researches to date." At long last, the true confessional of the obscured goings-on behind the blue-collared apron is off the press. So we are told. Like the workingman's wife who, purportedly out of self-confessed ignorance and fear of being tricked, places her trust mainly in national advertising, we are similarly to prepare ourselves to learn the truth from this team of professional researchers at Social Research, Inc.

Now just what is the truth about the wage earner's wife? For its proper appraisal we need first inquire into the method of its gathering and the manner of its communication.

Scores of questions on a variety of topics, phrased in alternate forms, scattered through three interview guides, varying as to structure, and interspersed with TAT pictures and sentence completions—were put to several hundred youngish working-class mothers and to a smaller number of middle-class mothers in four cities. As though

this alone were not sufficiently impressive an undertaking, additional topics were brought out for consideration by reference to the results of more circumscribed studies that had been conducted over the years by the same organization.

The authors provide observations and interpretations in a crisp, readable, nontechnical language. Apparently they elected to rely mainly on quotes from the wives themselves, for their use of statistics is at most subliminal; even percentages are rarities.

THE workingman's wife is called upon to inform us about her daily rounds; her relations with husband, children, relatives, and friends; the nature of her participation in voluntary associations; her budgeting and purchasing habits; her esthetic views; her projected hopes and fears. She emerges as a woman needing, and seeking to buy, constant reassurance that the significant people in her life, like her husband and children, love her. She reveals herself as a person who is fearful of the future, who anticipates exploitation and devaluation at every turn, who is preoccupied with physical safety, and who surrounds herself with those ornate, garish, and impractical furnishings which she professes to abhor. In summarizing, the authors provide suggestions as how to best reach this woman with advertising and sales talk.

The characterization is striking. Yet it is sometimes difficult to distinguish her from her middle-class counterpart since the presentation shifts back and forth between the positive and the comparative. Moreover, an inescapable feeling

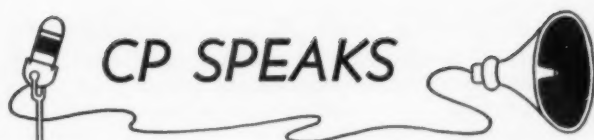
emerges that there is much in the book which is circular. For example, the measures of working-class status were such as to select families where the standard of living (unlike that in the middle-class families) had reached its peak and would soon decline, and where the educational backgrounds of husband and wife were relatively low. It is not surprising, therefore, to find the workingman's spouse concerned with economic security, pessimistic with regard to her future, and provincial in her shopping habits.

It would appear from the authors' remarks that the workingman's wife is neither emotionally nor intellectually a superior person, nor capable of 'positive thinking': The "tin-can economy . . . may be the best device for these women, given their habits of mind and will-power"; "they equate 'interest' with old-fashioned concepts of usury"; they show a "peculiar concern with the safety factor in a house's construction" and "depression phobia." If it is true that jerry-built housing, false advertising claims, usury, and economic depressions are things of the past, then one would indeed have cause to view this woman's 'personality' with some concern.

It is also fair to ask: Is the woman in the portrait *truly* the workingman's wife? The husbands of most women who read *True Story*, *True Romance*, *True Experience*, or *True Love Stories*, hold blue-collar occupations. Over two-thirds of working-class housewives who read magazines at all read such magazines. On the strength of these estimates, the authors attempted to interview women (a "goodly number" were unavailable) whose names were supplied by Macfadden. (Middle-class wives, on the other hand, were selected through quota sampling in substantial residential areas.) One cannot tell whether these blue-collar wives differ from non-Macfadden-reading working-class wives only in the media employed for fantasy gratification.

A more thoroughgoing study of the tantalizing woman in the portrait, and of the important generalizations made about her, still needs to be made.





CP SPEAKS

KEEPING THE RECORD STRAIGHT

FOR a long time *CP* has challenged its readers to name a book that *CP* has not reviewed and that should have been reviewed, and for a long time no one picked up *CP*'s gauntlet. But now at last this winter *CP* has got three of its gauntlets taken up. And that is fine. *CP* ought to be in such close communication with its readers that they can challenge or upbraid it. Here is what has happened, with the quotations distorted just enough to preserve anonymity.

(1) There was a book of which the consultant said: "This is a rather wordy bit of armchair philosophizing, including randomized criticisms of Freud and the behaviorists. It was written, I would guess, from lectures, and I think it is aimed more at philosophers than at psychologists. The author has very little of his own to contribute. I am probably being unkind, but I do not think it deserves a review in *CP*." Well, that is the sort of considered criticism that *CP* trusts when it comes in from a competent consultant. So no review.

Exactly one year later to the very day came a letter about this very book from an eminent psychologist whom *CP* respects. Was not *CP* going to review the book? He thought *CP* should, would be glad to do it himself if *CP* had no one else. *CP* checked with another consultant, equally competent in the same field, and then told the would-be reviewer to go ahead. *CP* would make a judgment with the review in hand as to whether the reviewer or the consultant had been right, but of course those cards were stacked in favor of the reviewer who has already read the book, and against the consultant who had only examined it.

Thus once again is there brought out into the light the idiosyncrasy of value judgments by authors, by reviewers, by consultants, and by *CP*'s own precious

editor. Idiosyncrasy is not to be regretted, dear Reader. The mature mind welcomes it, having the capacity to suspend judgment, to entertain doubt, and to enjoy dissent which is one of the forces that keep the intellect awake.

(2) Of book no. 2 the consultant had said: "No review, I should say. Too many topics for one volume. None is given a sufficient thorough-going treatment to be of help to the practicing or teaching psychologist." Ah, that's *CP*'s Achilles' heel. It does not know when to review poor books unfavorably and when to ignore them. (See *CP*, July 1960, 5, 220f.) It is quite sure that it cannot review all books that consultants think are poor (*CP*'s no crematory), that it must review some poor books in maintaining its critical function (especially bad books by distinguished authors), and that the borderline between *Yes* and *No* is so wobbly that there is no chance of unanimity on the dubious cases. What would you do, Reader, if you had to solve this problem with no extra space at your disposal?

(3) Then there was a third book that dealt with certain broad social problems in a manner which led the consultant to say: "It is written for the general public and takes an enlightened stand but introduces as evidence nothing more than the author's assertions. I would suggest that no review be published." So again no review.

The congeries books—collections of papers, reports of symposia—make trouble. *CP* tells a reviewer to study such a book and, if he can find no unity in it, to recommend no review. *CP* does not print abstracts of papers. It is, however, surprising how many reviewers have found unity in what seemed to be a mere congeries. As yet there has been no complaint about *CP*'s skipping a congeries book, but just wait. *CP*'s readers are getting less bashful.

BOOKS TO COME

BEFORE spring comes the Ronald Press should have out J. McV. Hunt's *Intelligence and Experience*, a book which deals with the transformation of the concept of intelligence which has been taking place during the last decade and a half and is not yet complete. The transformation is from the belief in fixed intelligence and its predetermined development to an acceptance of the role of experience and of central neural events as the more proximate conditions of intelligence, even though the genes may still be supposed to fix the limits within which experience can act. The volume reviews the history of the concept, examines the investigations of learning-sets and problem-solving, considers the strategies of computers and of human brains, takes a long look at Piaget and his collaborators, marshals all the evidence, and then does a little prophesying. That is enough for Hunt in 1961, but there is a pretty good chance that another book, now more than half done, a companion volume called *Motivation and Experience* by the same author, will turn up in 1962, and maybe—now that we have started prophesying—there will be a third book in 1963, a book about rearing children, the chapters that got squeezed out of the first two books because they grew too rapidly. Wesley C. Becker is helping with No. 3.

SOME of the friends of Joseph Church are excited about his *Language and the Discovery of Reality: a Developmental Psychology of Cognition*, which Random House expects to publish in March or April. The story of the book and of Church is that Church went first to the New School, then to France, then to Cornell, where he discovered R. B. MacLeod or was discovered by him. MacLeod introduced him to Merleau-Ponty's *Phénoménologie de la perception*, which had so much to say about Heinz Werner that Church went to Clark, and then, MacLeod and Wernerized, to Vassar (where he is now) to start a course in the development of language and concepts in children and an empirical longitudinal study of this developmental process in children from birth on. That work goes on, but the book insists on be-

ing written now because Church thinks it is different from the standard authors in this field, sticks closer to the actual live child, and says things about symbolic and presymbolic thinking and the persistence of infantile modes of thinking that will, Church both believes and hopes, shock a lot of people out of complacency—provided they understand him. Wait and see. Authors ought to have faith and euphoria and Church has.

—E. G. B.

Dream Kaleidoscope

Manfred F. DeMartino (Ed.)

Dreams and Personality Dynamics. Springfield, Ill.: Charles C Thomas, 1959. Pp. xviii + 377. \$10.50.

Reviewed by JEROME L. SINGER

who is a clinical psychologist and a psychoanalyst in private practice in the New York area and principal investigator in the National Institute of Mental Health's project on fantasy and imagination, sponsored by Teachers College, Columbia University.

HERE is a book with a title which, like the fabled "Lincoln's Doctor's Dog," should have an immediate and wide appeal to a large audience. What a disappointment when the book turns out to be another minimally-edited compilation of an uneven group of published articles and excerpts! Manfred F. DeMartino, who is at present School Psychologist in the Board of Cooperative Educational Services in Onondaga County, New York, has apparently used some of the material incorporated in this book in his teaching at Syracuse University and at Alabama Polytechnic Institute. He has himself carried out some library research on children's dreaming which is included as one chapter of this book. If any pattern or integrating principle is discernible in this book it probably reflects an influence on

the editor of the continued concern of Dr. Calvin Hall with making sense of the phenomenon of dreaming and providing meaningful operations for its study. But Dr. Hall cannot be held responsible for the many questionable features and omissions of this book.

The editor's purpose is to provide material of interest and value for clinical psychologists, students, academic psychologists, and practitioners of dream analysis. A rather pedestrian introductory chapter does little more than summarize the content of the articles with almost no attempt at integration of the studies within a broader framework or a critical evaluation of occasionally contradictory points brought out by the papers. The articles themselves include a masterly excerpt on dreaming from Gardner Murphy's *Personality*, detailed presentations of a number of loosely connected empirical and experimental studies on individual differences in dreaming and relationships between dreaming and projective materials, some well-executed papers on the testing of psychoanalytic hypotheses and on the cognitive function of dreams by Calvin Hall and his students and, finally, a lengthy reprinting of the important studies by William Dement and Nathaniel Kleitman on the relationships between sleep, eye movements, and dreaming. Interspersed with a few careful and thoughtful papers are rather rambling, old-fashioned and unconvincing clinical articles by Otto Kant and Medard Boss, among others.

The omissions from this book are glaring. While the failure to include Freud's papers can be understood, following the editor's reasoning, the absence of systematic papers by any of his successors in psychoanalytic theory is indeed lamentable. Why nothing by Erik Erikson or T. M. French, men who have devoted years of concentrated thought to the issues raised by Freud? One looks in vain for any really thorough history of the trends in modern dream interpretation and in the psychology of dreaming presented with technical detail and criticism. Instead we find a dull bibliophilist account of the history of popular dream books and a more scholarly treatise on ancient dream interpretation (by Harold Mc-

Curdy) that terminates with Synesius of Cyrene in the fourth century.

WHAT purposes can this book serve? For the student it may afford some opportunity to see collected in one volume some examples of empirical studies of dreaming. Any serious instructor is more likely, however, to prefer to make his own selection and refer students to the original papers. For the researcher in this area this book will be dismaying since most of the excerpts will tease him by referring to articles without any formal citations. The failure to systematize references or to at least leave the authors' original listings is a serious editorial lapse.

It is also difficult to see any use that this book may have for practitioners. The few papers that deal with clinical approaches to dream interpretation are of questionable interest and lack the detailed and intensive scrutiny of issues that may be found in the work of Ella Sharpe, Montague Ullman, E. S. Tauber, Erich Fromm, Robert Fliess, and Erikson to mention just a few persons who have written in this field. The juxtaposition of some loose clinical thinking and the uninterpreted Kinsey report excerpts is confusing at best.

The editor has devoted the last 53 pages to a reprinting of the Dement and Kleitman and Dement and E. A. Wolpert papers which, pending verification with larger samples, may represent a significant methodological breakthrough in the study of dreaming and sleep patterns. Bringing these experiments and some of the work of Hall and his students together under one cover as examples of empirical approaches to dream research is worthwhile. Yet, most of the valuable excerpts are from the easily obtainable standard journals. While well-edited, organized collations of instructive published papers may occasionally be useful, *Dreams and Personality Dynamics* seems unlikely to prove valuable enough to merit the attention its title will attract.



Leisure without books is death and burial of a man alive.

—SENECA

From The Century Psychology Series

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By **Gregory A. Kimble**, *Duke University*. Retaining essentially the same point of view, virtually all the material in this revision is new. The approach is factual, historical, and non-theoretical; the emphasis is on placing facts and alternative conceptions of the nature of learning in an organized pattern. The book includes 180 graphs and tables, a bibliography of 1500 items, and a glossary of 100 technical terms.

Just published, 680 pp., illus.

USING TESTS IN COUNSELING

By **Leo Goldman**, *Brooklyn College*. Assuming a knowledge of elementary statistics and the basic principles of tests and measurements, this new text is devoted entirely to the use of tests in counseling. The book discusses the counselor's selections of tests, the psychology of test-taking, the interpretation of test results, and the communication of the results to counselees and other interested persons.

To be published in April, about 480 pp., illus.

VERBAL BEHAVIOR

By **B. F. Skinner**, *Harvard University*. Following the pattern of a functional analysis of behavior, Dr. Skinner's definitive work offers a systematic formulation of verbal behavior (the behavior of the individual speaker and listener or writer and reader). The book discusses the conditions responsible for the acquisition and maintenance of various kinds of verbal behavior as well as the effects of multiple variables—both in selecting particular forms of response and in probing verbal behavior clinically.

478 pp., illus., \$5.50

TOWARD UNDERSTANDING HUMAN PERSONALITIES

By **Robert Leeper**, *University of Oregon*; and **Peter Madison**, *Swarthmore College*. Based primarily on the work of experimental psychologists concerned with problems of learning, concept-formation, perception, and motivation, this text emphasizes the psychology of normal persons. Following five chapters of concrete material, the main psychological aspects of personality are dealt with in more generalized terms.

439 pp., illus., \$5.50

Appleton-Century-Crofts, Inc.

35 West 32nd Street New York 1, New York

Philosophy or Statistics?

F. V. Smith

Explanation of Human Behaviour. (2nd ed.) London: Constable and Company, 1960. Pp. xii + 460. 30s.

Reviewed by WILLIAM STEPHENSON

Professor Stephenson, a graduate of Durham University in England, is now Distinguished Research Professor at the University of Missouri. For many years he was Reader in Experimental Psychology at the University of Oxford, and before that an assistant to Professor Spearman at London. In the United States he has been a Visiting Professor at the Universities of Chicago, California, and Washington. He is author of *The Study of Behavior: Q-Technique and its Methodology* (Univ. Chicago Press, 1953) and expects soon to publish *Intimations for Self-psychology, as well as a general theory of mass communication*.

F. V. SMITH is professor of psychology at Durham, third oldest of the Universities of England, a group of small Colleges nestling round the towering medieval Cathedral, symbol of an ancient palatinate. It is the *alma mater* of the present reviewer, which serves as a reminder that, in England, psychology is not dominated by postgraduate departments—it remains in the hands of scholars at the College level. The professor and his students are, therefore, free to be retrospective, to find time to read Wertheimer, Stout, and others whose names are unlikely to appear in the *genera trivialis* of American College textbooks. It is all very much as it should be.

But there is purpose in this book over and above the scholarly exposition of the psychological systems of McDougall, Allport, Freud, Lewin, and of Gestalt and Behaviorism (Watson, Hull, Tolman). The first four chapters discuss scientific *explanation* as a basis for critical appraisal of the systems. *Explanation* of human behavior, Smith concludes, can be neither causal (as

material cause) nor deductive. Instead, it has only "approximate resemblances" to these modes. The man with a toothache, visiting a city, may be going to a dentist—or he may not. One cannot predict, whatever we may do by careful observation. But out of a hundred men with gumboils a significant number (statistically) will be on their way to dentists—of this we may be sure. And laws and 'regularities' in the psychology of human conduct are of this statistical nature. The statistical argument, Smith considers, is to be regarded as a "definite type of explanation" because it can be subjected to testing and verification. This fits the modern formula that explanation is a conclusion to a tested hypothesis. Besides, it is useful, almost too obviously so, as a face-saving device.

Thus, after ably discussing Freud's system, much of which might be of dubious scientific worth without this little bit of logic, Smith has to say: "Correlations indicating a high degree of probable association are, however, possible, and it is interesting to note that for some features of Freudian theory small correlations have been established." Thus does statistics save the day! One understands, therefore, how favorably Meehl's conclusion is received, that statistical prediction is more accurate than clinical.

THIS reviewer suggests that serious misunderstandings of logic-of-science are here involved. Before dealing with these, however, it is important to observe that, after giving the reader as able an exegesis as the psychological systems are likely to have for a long time, Smith concludes that an era of system-building is over, and that psychology is now engaged on testing hypotheses and gather-

ing empirical facts. Notwithstanding the statistical face-saving device, surely the conclusion is also that the systems, of McDougall to Tolman, with the others in between, are, plainly and straightforwardly, *failures*. The scholar should ask why this has happened, and it is not enough to blame it on the complexity of human conduct, or to seek a way out via statistical logic. After all, Smith could have found out where the man with the toothache was going, simply by asking him. Perhaps psychology, in all the systems so ably reviewed, has likewise been putting no pertinent questions to human nature.

Such, indeed, is undoubtedly the case; but first as to the statistical matter. If anything has advanced in the past few decades it is our understanding of logic-of-science, an understanding not reflected in Smith's hundred pages on explanation. There is a failure, for example, to distinguish *ad hoc* from *genuine* hypotheses. There is no understanding of what *operations* are really about, and Smith thinks of 'regularities' in the same sense as *invariances*. Obviously there are countless practical rules, as Smith reminds us, that make possible some approximate forecast or prediction of human behavior, but they are not on that account necessarily of interest to science. Apart from being *selective*, science draws a distinction between 'regularities' and *invariances*. The latter are indeed difficult to find in psychology (as elsewhere) to judge by Spearman's abortive effort after *g*, but the situation is quite different for *laws*, which are *used* to help the scientist find his way about. The logic is that *if* certain facts occur, they will have such and such an explanation. Smith seeks for regularity in diversity (statistically), but the logical possibility exists of finding diversity in regularity—if we may coin a paradox. A *law* is presumptive as far as 'regularity' is concerned, but the *explanation* can only be given after the facts are observed. The emphasis is on operations providing the facts. Explanations are then discoveries, not merely conclusions to tested hypotheses. But this, no doubt, is too much for easy digestion, and we may merely recommend it to Smith, along with references to C. S. Peirce, to Hempel, Fiegl, Pap,

Popper, and to others who find no mention in his opening chapters.

THE conclusion that an era of system-building has ended should have led the scholar to pause, to wonder why. Smith considers the case of a native, observed to be creeping through undergrowth—he might be wife-hunting, avoiding enemies, stalking deer, or something else. His personal prestige may be involved, perhaps of his tribe and family as well. Physical, neurological, and biochemical factors are undoubtedly also involved. So the systems pass before our eyes, from physiological to social. We are told that "the actual reasons for his behavior could only be known by inference after prolonged observation or by consulting the man himself who could give reasons for his behavior" (p. 118), yet

it is true that *nowhere in all the systems is such a man, homo sapiens, whether native or cultured, ever really consulted as to reasons for his behavior.* The fundamental failure of the systems, and of the present degradation to hypothesis-testing for facts, is purely methodological. The great English scholar, James Ward, tried to warn about it, as Kierkegaard had done a hundred years earlier in his *Concluding Unscientific Postscript*. Scholarly detachment might bring Kierkegaard, Dilthey, and James Ward into the picture, and thus help to set psychology on its way again, systematically, to understand human behavior from the standpoint of the centrality of the subject, as Ward suggested. The methodology of studying everything *else* about him has apparently signally failed.

The biochemical and the clinical discussions are mostly centered upon certain compounds or groups of compounds. In some chapters, however, the topic refers to a particular group of diseases or symptoms (e.g., coma) or, as in Chapters 19 and 20, to a certain organ (i.e., muscle).

THESE critical remarks refer solely to the formal aspect of this collection. The content of the book is excellent. The first two chapters deal with vitamin-B disorders. They are written by Sir Rudolph Peters and J. D. Spillane. Some historical remarks and tables add considerably to the enjoyment in reading Spillane's essay on the clinical aspects of these diseases. R. H. S. Thompson presents in Chapter 3 some experimental evidence that enzyme disorders may be the cause of demyelinating diseases. This very informative paper is followed by Sir Charles Symond's presentation of the clinical, hereditary, and pathological aspects of multiple sclerosis and some of the other demyelinating diseases. C. Rimington and J. St. C. Elkington (Chapters 5 and 6) deal with the biochemistry and clinic of the disorders of the porphyrin metabolism. The reader, interested to learn more about porphyria, may regret that both of these otherwise excellent chapters do not have a list of references. The role of calcium and phosphorus in the normal and pathological physiology of neuromuscular transmission are discussed by W. W. Payne in Chapter 7, and P. K. Robinson follows in Chapter 8 with the clinical consequences of disorders in the calcium and phosphorus metabolism, emphasizing the fact that changes in the behavior of these two elements may affect the nervous system in two basically different ways: first, by a direct effect on excitability, and second, indirectly via compression exerted by disformed parts of the skeleton. Cumings and Reginald Kelly discuss in the next two chapters the biochemistry and clinic of the copper metabolism with the main emphasis upon Wilson's hepato-lenticular degeneration. In view of some recent findings that caeruloplasmin or related substances may be involved in the causation of mental disease, some more detailed information about this copper-

Biochemical and Clinical Integrations

John N. Cumings and Michael Kremer (Eds.)

Biochemical Aspects of Neurological Disorders. Springfield, Ill.: Charles C Thomas, 1960. Pp. x + 230. \$8.75.

Reviewed by WERNER P. KOELLA

Dr. Koella, the reviewer, is Senior Scientist in the Worcester Foundation for Experimental Biology in Shrewsbury, Massachusetts. He holds an MD from the University of Zurich, where he taught physiology for seven years. In America he has been attached to the University of Minnesota and the University of Louisville before assuming his present post. During the last fifteen years he has published almost three score physiological researches on a wide range of topics.

THE story presented in this 230-page volume is the substance of 20 lectures given in 1957-1958 at the Institute of Neurology in the National Hospital, Queen Square, London. As the lectures were delivered by 19 different authors, all specialists in their fields, the booklet to emerge had to be, by

necessity, a multi-author opus—with all its advantages and disadvantages. The editors (J. N. Cumings and Michael Kremer, both also authors) had the happy idea of presenting 20 chapters in 10 pairs, putting together a paper on the biochemistry of a particular area and a paper on the clinical application. Such pairing, if it is well handled and if the two presentations are well coordinated, results in a highly integrative picture, and here this integration is very well accomplished. Perhaps, some of the co-authors of these paired chapters should have tried harder to avoid repetitions in describing basic facts, and to eliminate re-citation and re-elaboration of the same references. Such repetitions are not out of place, in an oral presentation; in a book they are superfluous. The selection of topics in the various chapters is not entirely consist-

protein compound with oxydase activity would be desirable.

In Chapter 11 N. F. MacLagen discusses the biochemistry of the diabetic, uremic, hypoglycemic, hepatic, anoxic and drug-induced coma, thus somewhat deviating from the general plan of the book. Apart from this slight inconsistency the chapter is, however, very well written and is a well of information for the biochemically interested clinician. Kremer in Chapter 12 confines himself to an excellent presentation of some neurological symptoms associated with liver disease. Chapters 13 through 18 by Derek Richter and Denis Williams, and F. T. G. Prunty and J. D. N. Nabarro, and Cumings and Paul Sandifer deal with anoxia, hypoglycemia, disorders of the adrenal and pituitary, and disorders in the lipid metabolism. B. McArdle and John Marshall, rather than to center their discussion on certain chemicals, focus their presentation upon an organ, the muscle, and its (complex) pathobiochemistry. In these two excellent concluding chapters the reader is led via the physiology of neuromuscular transmission to a masterly discussion of myasthenia gravis, polymyositis, and benign congenital myopathy.

Each author in this book has very well presented the highlights of his own work and of related studies in a not too technical fashion. The background in biochemistry and neurophysiology as taught in the basic science courses in medical schools is sufficient to follow the chemical papers. The practicing neurologist will find this book most valuable as it offers an excellent opportunity to deepen his background and understanding of the etiology and pathogenesis of a number of neurological disorders. For the young biochemist the book is especially stimulating as it demonstrates the value of basic biochemical research to understand, treat, and prevent disease. The psychiatrist and psychologist reading the different chapters may gather new hope that some day biochemistry will be able to put its finger upon the metabolic lesions causing mental diseases. The stories of some vitamin-B disorders, myasthenia gravis, Cushing's disease, and phenylketonuria, just to name a few, should be encouraging.

The Trees that Hide the Forest

Herbert Kaufman

The Forest Ranger: A Study in Administrative Behavior. Baltimore: Johns Hopkins Press, for Resources for the Future, Inc., 1960. Pp. xviii + 259. \$5.00.

Reviewed by WARREN G. BENNIS

who is Associate Professor of Industrial Management at the Massachusetts Institute of Technology. He got started on his present interests in social organization and control with Douglas McGregor and Irving Knickerbocker at Antioch College, and then took a PhD at MIT with Herbert Shepard and Alex Bavelas. Then Boston University's Human Relations Center and Dept. of Psychology. Bennis, K. Benne, and R. Chin are working now on a book about planned change, one which Holt is going to publish in 1961 maybe.

SOCIAL scientists have an awesome capacity for destroying our nostalgic images of the few remaining exotic and romantic 'callings.' Taxi-dancers, call girls, boxers, jazz musicians, and others have all relinquished their stubborn patina to the stark categories of sociological and psychological analysis. And now—*mirabile dictu*—the Forest Rangers have surrendered. The disenchantment is abrupt and shocking. For Herbert Kaufman, an Associate Professor of Political Science at Yale University, has written a book which attempts to examine the processes by which these last holdouts of a pioneer age—peering through their binoculars on a lonely mountain top, so my stereotype goes—have been traduced into the inexorable alchemy of our age: Organization Man. This book, then, is essentially a study of social influence; a book which describes how various persons with differing predispositions and backgrounds come to be socialized and homogenized, so as ultimately to conform to the organizational requirements. And

this process occurs *despite* a set of unique circumstances of the Forest Service: geographical dispersion, varied job functions, inconsistent directions, attitudinal barriers, individualistic ethos, diverse local pressures, all of which make up what Kaufman calls "centrifugal forces" and "tendencies toward fragmentation."

The question that Kaufman sets out to answer is why such strict conformity obtains under conditions which would seem to lead to independent action, deviant behavior, informal activities, and the usual 'snafus' which honeycomb the typical field-headquarters type of agency. This is an exciting question and far from a trivial one. Quite apart from the practical illumination that it provides for the uneasy practitioner who administers these sprawling behemoths, this account is one of the first organizational studies that attempts to probe into the specific control mechanisms that modify, modulate, and control behavior—the specific organizational dynamics by which William H. Whyte's specter becomes a reality.

Kaufman attacks his problem by combining anthropological techniques with his own orientation in political science, an example of the interdisciplinary nature of the emerging field of organizational behavior. After first familiarizing himself with the history and practices of the Forest Service—through its central office in the Department of Agriculture—he spent a week's time at each of five different Ranger districts. (Ranger districts "are the smallest geographical subdivisions in national forest administration, and the district Rangers who head them are the lowest ranking professional offices commanding administrative units.") During this week, Kaufman spent almost full time with the Ranger, observing, questioning, probing, challenging, and spending late evening sessions with the Ranger at home. In some cases, he engaged in long 'conversations' with subordinates and chiefs of Rangers, as well as with local community officials. The five districts studied "present a considerable spread of conditions . . . and the Rangers themselves constitute a variegated sample of field offices." (As of 1958 there are 792 Ranger districts scattered

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April 1961, 768 pp., \$7.95 tentative

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January 1961, 525 pp., \$8.00 tentative

Clinical Inference and Cognitive Theory

THEODORE R. SARBIN,
University of California, Berkeley
RONALD TAFT, *Univ. of Western Australia*
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An original theory of the inference process developed here adds a new dimension to the consideration of diagnosis construction and personality assessment. 1960, 302 pp., \$5.50

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J. P. CHAPLIN, *University of Vermont*
T. S. KRAWIEC, *Skidmore College*

Covering systems and theories in such areas as perception, learning, and personality, this book surveys the evolution of contemporary concepts. 1960, 473 pp., \$6.50

HOLT, RINEHART AND WINSTON, INC.
383 Madison Avenue, New York 17, New York

throughout 149 National Forests located in the 10 regions of the USA.)

The district Ranger's job consists of an array of surprising administrative and technical functions. Aside from the traditional fire-control duties, he is involved in and responsible for timber management, recreation and land uses, range and wild-life management, engineering operations, personnel management, information and education, state and private forestry, and fiscal control. To boot, the Ranger is at the vortex of a baroque administrative complex, confounded by conflicting purposes, contradictory and shifting demands, reporting and authority systems, rigorous inspection, etc. Each Ranger, for example, maintains a small library of service regulations. And yet Kaufman's interest was primarily inspired by the prevailing conformity—although the criterion of conformity is never clarified—under field conditions which would normally exacerbate deviation from policy at the local level. In short, in spite of seeming confusion, there is an amazing correspondence between policy directives as issued from Washington and Ranger conduct in the field.

This congruence of policy and action can be explained, says Kaufman, by certain "forces toward integration." These consist mainly of control mechanisms which can be manipulated by the organization and one other which the Ranger himself brings to the role. One integrating factor is what Kaufman calls "preformed decisions," a set of rules or programs established by higher headquarters, which provides specific guides for action, taking account of all logical contingencies. Variability of behavior is thus constricted to the extent that the Ranger can understand and follow the regulations. Secondly, there is an elaborate set of radar-type devices for detecting and discouraging deviation, such devices as reports, official diaries, inspections, frequent transfers (which heighten the notion of interchangeability of personnel), public appeals (which bring deviations to light), various sanctions, and 'feedback and correction' devices. The cobweb of control mechanisms is so exacting and complicated that it is easier to comply with them than to buck them. It would seem

doubtful that the Ranger could see the forest for the trees.

In addition to these forces, the organization—half intuitively and half for other reasons—employs a set of interesting social psychological devices for building 'identification' with the Service. These include the use of symbols (the required uniform), public relations, strong reference groups, post-entry training, etc. And finally, the conformity recipe is rounded off by stringent selection procedures which generally tap only those highly motivated and dedicated individuals who bring into the service a 'willingness to conform' in order to reach their 'calling.'

ALTOGETHER these forces lead to an unusual state of affairs referred to as "voluntary conformity," a situation in which Rangers for the most part operate in accordance with policy directives, not because they accept a set of rules slavishly—but because, it appears, that what the Rangers want is also what is wanted by higher headquarters.

It is interesting to consider "voluntary conformity" in relation to Argyris' work on the basic incongruities between personality needs and organizational requirements. By and large there have been two noticeable resolutions of this conflict: the Huxley-Orwell specter where the individual surrenders his autonomy to the Organization, where all responses are dictated and controlled, and the Huck Finn or Utopian resolution where individual expression and needs eclipse larger organization goals. The Forest Service's resolution is inter-

esting in this regard, for by selecting for entry ardent enthusiasts and by socializing the enthusiasts, an apparent fusion is obtained between organizational and individual goals.

This is an important book for providing source material for the dynamics of socialization with organization. It also presents an interesting resolution of one of our society's main problems—the individual in the organization. Its failings are more in terms of disappointments—not with what the book is, but with what it might have been. For Kaufman is a social psychologist *manqué*, and, while he edges up to viable social psychological analyses, he stops short of conceptualization when it is most needed. (Reference group theory, for example, would have found great use in certain sections. The footnotes are there, but the conceptualization is not.) This lack of sophistication shows up also in a number of unsupported and undocumented assertions about Ranger behavior and attitudes. Nor was I ever certain when Kaufman was reporting on the basis of his field informants or Washington policy, for no distinction of this kind was ever made and I was continually uneasy lest I was getting headquarters' perceptions instead of field facts. Indeed, the distinct impression I get is that Kaufman might very well have spent more time in the field and less at headquarters. For clearer and firmer evidence as to actual behavior—not thoughts about behavior by others—is required before we can fully understand the complicated, informal, and existential man confronting the organization.



The proof of self-evident propositions may seem to the uninitiated, a somewhat frivolous occupation. To this we might reply that it is often by no means self-evident that one obvious proposition follows from another obvious proposition; so that we are really discovering new truths when we prove what is evident by a method which is not evident. But a more interesting retort is, that since people have tried to prove obvious propositions, they have found that many of them are false. Self-evidence is often a mere will-o'-the-wisp, which is sure to lead us astray if we take it as our guide.

—BERTRAND RUSSELL



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128 pages Paper covers 8½ by 11 inches February 1961

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Child Observed or Child Inferred

Ruth S. Eissler, Anna Freud, Heinz Hartmann, and Marianne Kris (Eds.)

The Psychoanalytic Study of the Child. Vol. XIV. New York: International Universities Press, 1959. Pp. 433. \$8.50.

Reviewed by ZANWIL SPERBER

Dr. Anna Freud, daughter of the famous father and now well known in her own right, practices psychotherapy with children in England. She wrote The Psychoanalytical Treatment of Children (Imago, 1946). The other three authors are all European-trained and practicing psychoanalysts in America. Dr. Hartmann's Ego Psychology and the Problem of Adaptation has been translated from its original German of 1939 by David Rapaport (Internat. Univ. Press, 1959). Dr. Eissler contributed to The Psychoanalytic Study of the Child in 1949, and Dr. Kris in 1951. Dr. Sperber, the reviewer, is coordinator of research on cerebral palsy at the Children's Hospital of Philadelphia. This is a long-range project involving many children and continued from prenatal stages up to one year and sometimes seven years. See his review of Auerbach's Schizophrenia (CP, Aug. 1960, 5, 359f.).

ONE of the fundamental assumptions of psychoanalytic theory is that the adult personality is best understood in terms of a developmental frame of reference. As Sigmund Freud, the therapist-researcher, followed his patient's verbalizations of "whatever comes to mind," therapist and patient invariably surveyed the patient's life. It appeared clear to Freud that the adult patient's symptoms were summary representations of a historical process originating in his early life experiences. The theoretical structure outlined by Freud focused on the sequence of life experiences as they unfolded in the earliest years of the growing human being, although the evidence from which the de-

scription of a sequential unfolding was derived originated in the memories adult patients produced in the course of psychotherapeutic work. That is to say, the developmental scheme is based on recall and inference (reconstruction) rather than direct observations.

The volume here reviewed is fourteenth in a series which, by title, would appear to be committed to an attempt to validate the deductions based on reconstructive efforts and amass new evidence by direct observation of children at the ages when they are presumably going through the critical phases of development. Most of the authors of the seventeen papers contained in the book are well-known psychoanalysts who publish widely in psychoanalytic journals. K. R. Eissler, Anna Freud, Phyllis Greenacre, and others whose contributions have frequently appeared in past volumes of this series are represented again. In the tradition of Freud, who broadly defined the material relevant to psychoanalytic theory, the authors are interested in many problems: creativity, the psychological impact on women of pregnancy, factors in therapeutic success and failure, determinants of artistic appeal, processes by which a sense of identity is established, etc. Although the breadth of the contents guarantees something of value to readers with literary as well as clinical, theoretical, and research interests, the reviewer was disappointed to find that material explicitly based on work with children appeared on less than 100 pages.

The children who are treated and studied by the authors of the case reports provide vivid examples of severe pathology illustrating fundamental prob-

lems which must be solved if we are to understand personality development. These are children who are so severely disturbed they are uncertain of where they end and where the external world begins. Their behavior clearly demonstrates the necessity for a secure anchoring sense of self, if a capacity for organized, directed behavior is to be achieved.

Although not presenting developmental material, Dr. Leo A. Spiegel makes an excellent theoretical contribution to our understanding of how the sense of self is established. His paper is *The Self, the Sense of Self, and Perception*. Integrating the experimental psychological findings of H. Helson and H. A. Witkin, Spiegel treats the concept of self as a frame of reference formed by the steady accumulation and pooling of specific "self-representations." These self-representations result from experiences of hunger, wetness, kinesthetic reactions to rocking, etc. If the adult caring for the neonate filters the environment properly so that internal stimuli (biological needs) and external stimuli will not impinge upon him too erratically, intensely, or frequently, then a steady pooling of self-representations will occur and lead to a stable sense of self. This stable self will then form the ground (frame of reference) in relation to which the developing child is able to establish clear differentiations and perceptions of others. Cross reference to cases presented (e.g., the chapter by Drs. Elkisch and Mahler) and examination of the factors associated with the reported instances of therapeutic success and failure appear to confirm Spiegel's outline of the critical dimensions for the establishment of a functionally adequate sense of self.

The work reported by Dr. Augusta Alpert on *The Reversibility of Pathological Fixations Associated with Maternal Deprivations in Infancy* is theoretically relevant and therapeutically promising. She works with children who as babies lacked a stable caretaking person who could guarantee adequate gratification of their fundamental biological needs. By nursery-school age these youngsters appeared to be autistically withdrawn. Devoted attempts to supply Corrective Object Relations (COR)

Psychology

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That intense involvements with these severely disturbed patients and exposure to the pressure of their needs in therapeutic interactions are not simple matters to handle is well demonstrated in the chapter by Rudolph Ekstein, Judith Wallerstein, and Arthur Mandelbaum. They describe the way in which the child's symptoms can act as a divisive force in the social structure of the treatment setting, and also as a source of psychological stress for a therapist. Their astute analysis should interest group dynamicists and sociologists, as well as clinicians.

It is encouraging to note the recent work of H. L. Raush, Allen T. Dittmann, and T. J. Taylor (1959) which indicates that complex therapeutic and interpersonal transactions, such as those described clinically by Ekstein, et al., can be subjected to empirical and quantitative research, a confluence of two research traditions which bodes well for the future of psychology as a science and clinical practice as a profession.

This reviewer has focused on the minority of pages dealing with the study of the child observed rather than the child inferred. Granting H. Orlansky's (1949) conclusion that no systematic correlation has yet been demonstrated between *specific* practices of child care, on the one hand, and adult personality, on the other, it nevertheless remains clear that the child is, indeed, father to the man.



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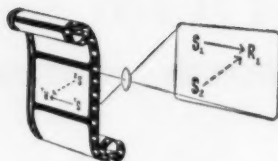
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INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



THE LOOK OF DISTRESS

A Two-Year-Old Goes to Hospital.

New York, New York: New York University Film Library. 50 min., black and white, sound. Rental \$10.00; sale \$150.00 (with guide).

Anxiety: Its Phenomenology in the

First Year of Life. New York, New York: New York University Film Library. 20 min., black and white, silent. Rental \$4.00; sale \$100.00.

Support during Labor.

Jackson, Mississippi: University of Mississippi Medical Center. 20 min., color, sound. Rental \$15.00; sale \$125.00.

Reviewed by WILLIAM KESSEN

who is Associate Professor in the Department of Psychology and in the Child Study Center at Yale University. Kessen took an ScM from Brown University and a PhD from Yale, specializing in animal learning at both institutions. His present interests center primarily around the human neonate and associated problems of emotion and anxiety in the very young infant. He says of himself: "I guess I belong to the 'new' generation of child psychologists who would like to study children without losing the child in the box or significant problems in the 'wonder of childhood.'"

THESE films share two goals: all three explicitly aim to instruct and to demonstrate while, in a much less obvious way, they seek to reform. Other communalities are harder to see; *A Two-Year-Old Goes to Hospital* is balanced and valuable, *Anxiety* is scattered and irritating, *Support during Labor* is very nearly bad.

James Robertson, skilfully and with restraint, has described the eight-day-long hospitalization of an English girl—Laura—in a manner which provokes, instructs, and moves the viewer. As part

of John Bowlby's research on the effects of separation of mother and child, Laura was chosen at random for observation from a list of children awaiting minor surgery (for Laura, the repair of an umbilical hernia); her behavior in the hospital ward is tracked day by day, from the initial insult of being bathed by a stranger to the incredulous joy of being dressed to go home. The camera is, of course, selective. It catches Laura's terror when she receives rectal anesthesia, her cool reception of her mother's brief occasional visits, the breakdown that comes when she incorrectly anticipates her discharge from the hospital. The resident drama of these scenes is heightened by the narrator's calm uninterpretative description and quotation. In addition to the record of these events of obvious impact, the film contains observations of the child's behavior at predetermined and regular intervals throughout her hospital stay. This 'time-sampling' device lends some plausibility to the subtitle, *A Scientific Film*.

Whatever its merit as systematic data, *A Two-Year-Old Goes to Hospital* is a convincing chronicle of human distress and of childish attempts to control the sources and signs of distress. By showing us a child too young to build all her defenses underground in a setting of high yet uncontrived stress, Mr. Robertson has exposed major issues in the study of human emotion. Perhaps the example of Laura puts most forcibly the problem of response analysis. She screams rarely but intensely, she sobs quietly, her face sometimes becomes distorted without tears, she sits or lies without animation, she speaks. Whether these manifestations are to be seen as representing a single underlying process or different systems of evocation and control, the psychologist of emotion must face the problem of measuring these expressions, however inelegantly,

before he may talk systematically and realistically about anxiety, or trauma, or distress. The variety of Laura's facial expression alone emphasizes the distance we stand from an adequate nomenclature and classification of emotional expression.

Hardly less clearly, *A Two-Year-Old Goes to Hospital* points to problems in categorizing and relating sources of distress and responses of control. Laura sees another child in the ward, asks why he is crying, then bursts into tears. She inquires about her mother's whereabouts with quiet control. The moment of her apparent greatest distress comes not on an occasion of pain or novelty but when she learns that she is not to go home at the time she thought she would. Corresponding variety exists in Laura's control of distress. Her mother's visits are not times of unmixed pleasure; on occasion Laura is reserved and unresponsive. She clings to her Teddy bear and 'blanket-baby'; she mauls a doll; she tells an observer, "My mummy is crying for me; go fetch her." The range of response seen in Laura puts current theories of emotional behavior to sharp test.

For the teacher of psychology, *A Two-Year-Old Goes to Hospital* provides a connected and credible demonstration of his discourse on stress, separation, anxiety, early defensive ma-



LAURA

A Two-Year-Old Goes to Hospital

neuers, and topics akin. He may be assisted by a guide that accompanies the film, by an article describing the case of Laura (Bowlby, Robertson, and Rosenbluth, 1952), and by a psychoanalytic review of the film by Anna Freud (1953). *A Two-Year-Old Goes to Hospital* is also a social document of honest power. Without preaching, it bears a message of reform and it may, through its influence on medical practice, moderate somewhat the cruelty of separating the young child in hospital from his parents.

QUITE different is *Anxiety: Its Phenomenology in the First Year of Life*. It is one of several films prepared by René Spitz as part of a "psychoanalytic research project on problems in infancy." Although the titles do not say so, the pictures were apparently made in one of the institutional settings referred to in Spitz' hospitalism papers (e.g., 1945), a circumstance that users of the film should note most carefully.

Part I of the film—called *Precursors of Anxiety*—purports to demonstrate circumstantial emotional behavior, weaknesses in Watson's theory of emotion, the unpleasure that arises in the third month of life when the infant is "deprived of his partner," and variations in the occurrence and character of smiling. The second section—*Anxiety Proper*—purports to demonstrate response to strangers where "the coloring of anxiety is unmistakable," and techniques for dealing with stranger anxiety, unusual but normal responses of anxiety, and finally, anxiety in three pathological states—"depression," "coprophagia," and "marasmus." This is a large order for twenty minutes and the consequence is an episodic, uneven film which is also a poor job technically.

The conjunction of Spitz' speculative titles and the raw presentation of infantile distress makes fully evident the suggestion of the Robertson film that our understanding is arrear of the facts. There is in *Anxiety* an unnerving sequence in which a young infant screams at the appearance of a human face; to relate this phenomenon to a "bad mother-child relationship" is to mock our ignorance. The titles distinguish verbally between the precursors of anx-

iety and "real anxiety" or "authentic anxiety" without a corresponding distinction in behavior; the inhuman character of the three pathologic cases shocks without instructing; the 'demonstration' that Watson was wrong leads to no alternative principle. The instructor is well warned to preview *Anxiety* closely before using the film; he will find it necessary to defend or to interpret much of its matter.

A Two-Year-Old Goes to Hospital comes close to transcending the case-historical limitations of narrative film, not because Laura is an 'ideal type' or because the setting is prototypical of stress (surgery may approach this status, however), but because there is enough detail and continuity in her story to illustrate the discussion of testable general propositions. The film is comparable to the description in detail of a new botanical specimen, species unknown. *Anxiety*, on the other hand, is a disturbing romp through the garden which tells us, at best, that the look of distress is various and uncomprehended.

THE third film, *Support during Labor*, looks pretty. Its apparent purpose in watching a young woman move through the last hours of her pregnancy is to convince physicians, nurses, and husbands that women about to give birth need "physical and emotional" as well as pharmacological support. Along the way, the film illustrates with competent photographic technique the use of controlled breathing, of changes in posture, and of massage to alleviate the discomforts of labor. As an aid to instruction about human emotion in a condition of normal high stress, *Support during Labor* is too bland and too flat to be of much value to psychologists.

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ON THE OTHER HAND



WOLMAN'S CONTEMPORARY THEORIES
AND SYSTEMS

It is admittedly difficult for an author to be impeccably objective about an unfavorable review of his book. However, Pastore's review of the author's *Contemporary Theories and Systems of Psychology* (CP, Jan. 1961, 6, 4f.) is quite different. For here we have a case of a book review that tells nothing about the substance of the book reviewed. The book's author presented specifically more than a score of psychological systems and offsprings of systems under the general heading of *Conditioning, Behaviorism and Purposivism*; more than a dozen under *Psychoanalysis and Related Systems*; and about a dozen as *Understanding, Gestalt and Field Psychologies*. Moreover, the author evaluated each system and offspring of system quite specifically—the larger ones in a special section of *Concluding Remarks*—and he 'stuck out his neck' in two concluding chapters of his own theorization. Yet in the entire review there is no single word which takes issue with, or says anything about, the substance of the presentations, the evaluations, and even the theorizing.

Presumably, the leitmotif and *raison d'être* of the review lie elsewhere, in such statements as (a) "the dependence of the author on secondary sources becomes evident"; (b) "errors in fact and quotations mar Wolman's text"; (c) the "text is faulty in presentation and exposition of material"; and (d) "it is unlikely that the author himself felt called upon to read Russell's and Whitehead's *Principia Mathematica*" (the book is included in the bibliography).

The last statement really merits no answer: the *Principia* was required reading in the author's doctorate course on Mathematical Logic, and it is one of six references at the end of the first chapter of Hull's *Principles of Behavior*. The statement "about faulty presentation" is based on nothing more than the author's presentation of Hull's theorems in the original before discussing them (not treating them as incunabula).

We might, therefore, begin with the charge of secondary sources made in the face of the book's more than 800 footnote-

references to 221 different authors and the author's reading all foreign-language source material in the original. Note the evidence: "the reviewer was able to establish 24 correspondences between pp. 422-428 and Humphrey's *Thinking* (pp. 1-149) with regard to phrasing, organization, or communality of quotations and references." Just what is the methodology of establishing undefined correspondences in phrasing and organization between six pages of one book and 149 of another when both deal with the same topic? The author tried "communality" and finds that of the 12 footnote-references in his six pages, those to Michotte, Claparède, and Murphy do not occur in the 149 pages of Humphrey; that one reference is specifically to Humphrey; and that the reference to Bühler is to a quoted personal letter. The Mills, Ward, Stout, Binet, Külpe, and Watt are common to the two books. It would indeed be surprising if they were not in a discussion of associationism in the nineteenth and early twentieth centuries.

Again: "In the remaining pages of the chapter (pp. 431-442) the relationship to Woodworth's *Contemporary Schools* and Hilgard's *Theories* is apparent." No evidence is adduced. All the author can say is that the statement is not true, nor is the allegation true—as is readily verifiable—about "the author's frequent omissions of page references to either quotations or theoretical points and inexact references to the literature." The charge of reliance on secondary sources is absurd. It begins with: "In the reading of Wolman's text the reviewer had a rather definite *déjà vu* feeling." Need it be said that "*déjà vu*" is a subjective phenomenon that seldom corresponds to objective reality?

The specific for the charge of errors relates to the empirical validation of Hull's theoretical statements: "The 121 should be 123, the 87 should be 86, Hull's book is titled *A Behavior System* . . ." Hull's statement (p. 352) reads: "Of the 123 propositions wholly or partially covered by empirical evidence, 106 or 86 per cent, were judged as substantially valid; 14, or 11 per cent, were judged as probably valid, and one proposition (related to Weber-Fechner law), or about 1 per cent, was

judged as definitely invalid." The reader will note that 106, 14, and 1 add up to "121," a number supported by an actual count of the propositions. Hence, the author used "121" instead of "123"; the reviewer obviously did not note the error in the original.

Other statements of evidence are: an error in the title of Hull's book, the use "definitely" instead of "substantially" validated, and four typographical errors. And that is all. The author whose creed considers the crow an unholy bird will, nonetheless, eat it for the sake of science, if any fair-minded reader will justify the branding charge.

Space forbids detailed consideration of such *jaccuse's* as (a) why the author who read Helmholtz and Zeigarnik in German cited the original references in the text footnotes (the English translations are given in the bibliography on pages 558 and 594, Ellis for Zeigarnik), (b) the need of exhaustive specific references in the text to pre-book publications of the experimental basis of some system when the systematic significance of the experiments is as a rule more evident in the system-maker's books (the author's book is a survey of theories and systems and not of experiments), and (c) why a discussion of Kris and Hartmann is omitted (see lines 9-12 on page x of Preface).

The reviewer's designation of the author as "a practicing psychoanalyst supplementing this activity by teaching" is hardly correct. The author's main occupation has for the last 25 years been the teaching of psychology, and he has taught it in four different languages. At present, the author, like the reviewer, is a full-time teaching member of the faculty of Queens College. It was said earlier that there is not a word in the entire review which takes issue with any substantive statement of the author's book. It may now be added that there is not a word in the entire review in which the book, adopted in less than half a year as a textbook in the graduate courses of a number of American universities is in any remote way commended. *Quot homines, tot sententiae*.

BENJAMIN B. WOLMAN
Queens College
Flushing, New York

A DANIEL COME TO JUDGMENT

The review of Dollard and Auld's *Scoring Human Motives* by Leary (CP, Oct. 1960, 5, 337f.) seemed to me a 'back-of-the-hand' kind of evaluation of what is an essentially significant book. Since I consider myself to be on friendly terms with

the authors and the reviewer, I think perhaps I can present in not too biased a fashion some of the more favorable aspects of the book which Leary did not mention.

The rather lengthy analogy of the Tom Sawyer and Huck Finn discourse was an amusing description of the validity problem which faces everybody working with research in psychotherapy, but I felt distressed that Dollard and Auld got the full brunt of the criticism, while some of the rest of us were less severely treated by comparison. Phenomenological observations are hard to objectify, but all scientific fronts are hard to pierce.

Leary devoted much of his review to a criticism of the title of the book. Undoubtedly many books are mislabeled; that may be one reason why reviewers are asked to tell what books are *really* about. While other products are often mislabeled too, I am not trying to justify a philosophy of *caveat emptor*. But is it really too inaccurate for Dollard and Auld to say that their book concerns scoring human motives? Some of their major categories are anxiety, dependency, hostility, sex, and love. H. A. Murray called these needs and presses, but is that so different a concept from motives? The authors were even daring enough to attempt to distinguish between the conscious and the unconscious. By my criteria, human motives certainly are involved in these concepts. I will grant that laughter, reasoning, psychosomatic symptoms, and sighing, are harder to fit under this rubric, but I feel it is demanding a lot to require that a title tell everything that a book encompasses.

I think one of Leary's main criticisms was that D & A require a lot of inferences to be made in the processes they describe, and they do not say so too clearly. He may be right, but we should not, I feel, overlook the positive qualities a book has because we find some aspects weak. Leary did not tell what the book was all about; he told the reader what it was *not* good for. I feel that anyone interested in analysis of verbal productions, particularly those in psychotherapy, would find this book a necessity. It is actually based on extensive research, and it gives valuable instructions for delimiting a 'unit' of protocol, and for applying a specific system of classification that is quite related to the sort of inferences about behavior which many investigators are interested in studying. There are two chapters citing impressive evidence in support of the reliability and validity of the method. (It is, of course, a weakness that only two investigators have been reported thus far in the validating data.)

It appears that we are often not approving enough of the research-oriented book, in contrast to the theory-oriented one. A lot of hard research work is sometimes considered pedestrian, while a small job of 'armchair and buttocks research' may be considered a great contribution. It seems to me that Dollard and Auld, like Leary, have managed to present an enviable combination of both types in their books, i.e., both work and thought.

The potential reader should be told, I think, that Dollard and Auld's book is, in general, a manual about coding categories, with extensive illustrations and practice exercises in their use. While there are 71 categories for patient's productions, many of these are really variations of fifteen principal classifications. It may be a weakness that there are only four important therapist classification categories, but the authors point out that many other investigators have offered systems dealing fairly effectively with the therapist's behavior.

Dollard and Auld also reviewed contributions by other investigators, and in a long appendix Dittes supplied a scholarly evaluative survey of previous studies bearing on psychotherapy interview content analysis. I was only a little distressed that D & A did not make mention of the theoretical dependence of their system on the need-system of H. A. Murray. This omission cannot be attributed to ivy league enmity; I suspect rather it is probably due to the fact that Murray's system is so fundamental that it can now be assumed to be a part of the psychological *Weltanschauung*. And Dollard and Auld describe one more practical application of that system, with variations, to the study of human behavior.

WILLIAM U. SNYDER
Pennsylvania State University

HUTT AND GIBBY ON THE CHILD

Stevenson's review of Gibby's and my book, *The Child: Development and Adjustment* (CP, Sept. 1960, 5, 296f.), makes two criticisms: (1) "misinterpretation of a number of studies in child psychology" and (2) "a failure to distinguish between hypotheses and facts." In addition there are some laudatory comments about the book. It is, of course, impossible to make a detailed rejoinder to each of the alleged limitations since this would take more space in CP than did the original review. Happily, the review offers quite similar examples of the two criticisms, so that we may take an example of each to evaluate the accuracy of the proffered judgments.

Stevenson offers five examples of "misinterpretation." Since the book contains

448 citations of the literature, one might conclude that the authors' 'batting average' is quite high. However, an examination of the examples indicates that they are taken quite out of context. For instance, the review comments: "It is good news, if true, that tests of linguistic skill and intelligence usually are correlated between .85 and .95" (p. 296). Now the book presents a careful evaluation of the characteristics of language development during the third year, and, in connection with this discussion, points up the high relationship between this *ability* and *intellectual development*, stating: "Linguistic skill and intelligence are highly correlated with each other, *providing environmental and social conditions are kept constant . . .*" (p. 159). The conclusion is based on *many studies* of linguistic and intellectual development and is especially concerned with factors influencing both. Perhaps the statement made in the *review* is challengeable; the discussion in the book, however, still seems to the authors to be entirely accurate and carefully considered. Hardly is it a misinterpretation.

The other criticism is, in the review's words, "a failure to distinguish between hypotheses and facts" (p. 296). The examples given concern the oedipus complex (italics mine), the relation of anal fixation to personality characteristics, superego and ego functions during adolescence, the relations of latency to abstract mental development, and the relation between regression and infantile speech. It is interesting that all of these examples relate to presentations of aspects of psychoanalytic theory. No similar criticisms are offered for exposition of other theories. More important, since each of the examples given clearly belongs to sections of the book in which personality theory is presented as *theory*, what possible basis in fact can the criticism have? Let me document a rejoinder with one example.

The review notes as an example of confusion of theory and fact, "the oedipus complex begins at three and ends at seven years" (p. 296). The book, however, never makes so flat a statement, not even of theory. In fact, it does not confuse, as the review appears to do, the development of oedipal conflicts with oedipal complexes. On the contrary, in presenting the theory of the oedipal problem, the book discusses the relationship of this problem to such items as factors in the familial situation, general maturational problems, class and other cultural differences, the effect of siblings and of the personalities of the parents. Finally, in presenting the *normative* aspects of oedipal problems and their reso-

lution, the book notes, "Toward the end of the third year most children will have become aware of anatomical differences between fathers and mothers" (p. 143). Throughout the discussions of this general problem, which is presented as a problem in interpersonal relationships, the theoretical issues are stressed, and when facts are introduced in support of theory or in criticism of theory, they are clearly differentiated from the theory. Certainly, there is insufficient factual evidence to support the statement made in the review. The book cites considerable evidence, however, to support the hypothesis that problems in interpersonal, and specifically in heterosexual, relationships do begin to become manifest, in general, during the preschool years.

MAX L. HUTT
University of Michigan

APPELBAUM ON APPELBAUM

CP's reviewer, John M. Butler, has claimed (CP, Dec. 1959, 4, 384) that my monograph (*Dimensions of Transference in Psychotherapy*) was not, as it purported to be, a study of patients' strongly held expectations of their prospective psychotherapists. "The principal aim of the study could not be achieved," Butler asserted, since it was not defensible to use the pretherapy expectation Q sort as a measure of strength of belief; one cannot estimate degree of conviction from a Q sort. Nowhere, however, did I claim that one could tell from this Q sort how strongly the beliefs it measured were held. It seems unlikely that these expectations were weakly held since the expectation types could be differentiated on the basis of degree of maladjustment (as measured by the MMPI), sex of patient, and duration of therapy (with suggestive differences in therapeutic outcome). Further, the design provided for a direct estimate of the strength with which the expectations were held: the same expectations-of-therapist Q sort was administered to the same patients after therapy (turning it into a description-of-therapist sort). It is in the nature of the variable that one cannot ask a patient what his transference attitudes are, much less how strongly he holds them. The strength of such beliefs can be estimated only through their susceptibility to change over time and in therapy.

Butler, in a subsequent letter (CP, Sept. 1960, 5, 316), does refer to this test-retest criterion, but finds the evidence insufficient. What was demonstrated by the substantial test-retest correlations, as Butler sees it, was "similarity," and this, he claims, "is

at a remove from persistent, tenacious, inflexibly unrealistic expectations." He also claims that this evidence for the persistence of patients' expectations may simply reflect the original accuracy of these expectations. To help the reader of CP evaluate these claims, I would like to offer the following summary of the relevant findings, as presented at the conclusion of the monograph:

"Evidence for the contention that the expectations measured by the Q-deck were strongly held was provided by: the relatively substantial test-retest reliability of the Q-sort with psychotherapy as an intervening condition; the related finding that, although the influence of the therapist's actual personality on the posttherapy Q-sort descriptions of him can be discerned, these Q-sorts also reflected a perseveration of the original expectations through therapy. Indeed, the patient's description of his therapist after therapy was more congruent with his pretherapy expectations than with posttherapy descriptions of the same therapist made by other patients after therapy" (p. 77).

Thus, the effect of experience with the therapist on the descriptions of him was demonstrated, but this lent increased significance to the discovery that the patients' descriptions of their therapists were more similar to their own pretherapy expectations than to the descriptions of other patients who had seen the same therapist. It seems reasonable to infer from these findings that the patients' expectations were held with marked tenacity, particularly since the intervening treatment (psychotherapy) is a procedure specifically designed to modify such expectations.

BERNARD APPELBAUM
University of California, Berkeley

(This letter is the fourth of the apfelbäumige Briefe. CP must now apply its rule of $r = 0.5$ for convergence: no response may be more than half as long as its stimulus.)

EVE JONES ON SHOBN

May I correct several incorrect statements in Dr. Shoben's review of my book, *Natural Child Rearing* (CP, Sept. 1960, 5, 276-278).

First, I am an instructor on the staff of University College of the University of Chicago, not a member of the Faculty in the Department of Psychology.

Secondly, I do have considerable experience as a psychodiagnostician of emotionally disturbed children and I am a psychoanalytically-oriented psychotherapist, but I

am not a children's therapist. Most of my practice currently is management consultation.

Dr. Shoben says that I have been "content to leave 'mental health' as the essentially undefined objective of child care." Actually, at many points throughout my book, I have indeed stated how each of the ego skills learned at given stages of psychosexual development is an essential and integral function of mental health, and in the last pages of my book, I have specified definitions of maturity and health.

I think my book does not, as Dr. Shoben seems to believe, fail to emphasize the role of cognitive faculties and of morality, for many specific sections discuss such morals-involved behavior as lying, stealing, cheating, bullying, licentious sexual behavior, etc., and many sections deal with helping a child recognize and conceptualize his psychological and physiological wants.

Let me also reply to Dr. Shoben's question that asks, virtually, whether we psychologists know anything at all about the effects on parents of reading a book like mine. I have learned from parents in family-life discussion groups that books, papers, and discussions on child-rearing practices have altered these parents' behavior with their children. For example, a majority of parents in the groups I led gave up such behavior as early toilet training, corporal punishment, and prohibition of private exploration of the sex organs—they stopped once they had had a reasonable opportunity to learn why this psychologist thought these practices are poor.

An informal survey of clinical psychologists with whom I am acquainted shows that the majority of them use corporal punishment, that many have deliberately "trained" their children to use the toilet and that many others do things like sleeping with their children or forcing food upon them. I conclude that the lay audience wants to and can learn from "experts" but the psychological experts themselves remain idiosyncratic and incorrigible.

It would, I believe, be informative to learn if a large-scale survey of the APA membership would support my informal conclusions.

EVE JONES
Chicago, Illinois

STENGEL AND COOK ON SUICIDE

The review of *Attempted Suicide* by Nancy Cook and myself (CP, Aug. 1960, 5, 248f.) says that on at least three major points the authors have failed to see or to say something which in fact they stated quite clearly and unambiguously.

The readers are told that the authors ignored the fact that their samples of attempted suicide were unrepresentative. The truth is that the authors stated this fact repeatedly and for this reason did not attempt a proper statistical analysis. The readers are also given to believe that in the authors' view the majority of people who attempt suicide are not sincere in their intention of ending their lives. In fact, the authors repeatedly emphasized that most people who attempt suicide are sincere in their self-destructive intentions, though those impulses may be vitiated by opposing motivations. The review fails to show that the authors deduced the appeal function of the suicidal attempt chiefly from its effects on and the reactions of the human environment to it, and not from the attempter's conscious motivations. The review is also defective in that it fails to give any hint of the results of the follow-up studies.

E. STENGEL
University of Sheffield, England

OBJECTIVE VS. PREJUDICED DISAGREEMENT

CP's brilliant little essay on book reviewing without tears or gnashing of teeth (Sept. 1960, 5, 290f.) can perhaps be briefly supplemented. In the same issue, I noted two frankly antagonistic reviews. A half dozen or so were adversely critical, but objectively so, in a way that would not cause old friends to avoid each other in New York next September.

The key to the difference seems to lie in a possible confusion of intellect and prejudice. In the antagonistic reviews it was clear that the reviewer did not like the book; he used his space to rationalize his dislike. In the others and in the vast majority of the reviews in CP, the author's objectives, his intended audience, and the difficulties of presentation are considered. The reviewer might have chosen a different but not necessarily a better path, but in any case he can appropriately point out the difficulties the author encountered and the omissions, overemphases and underemphases, and the like, as he sees them.

A reviewer will do well to realize that his critical review is actually a self-revelation, that he gives himself away as the kind of person who holds the beliefs and attitudes he so transparently reveals. It is interesting to note that in one of the antagonistic reviews, the editor recognized this fact and thoughtfully dulled the edge of the review in his introductory italics.

WM. CLARK TROW
University of Michigan

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METHOD IN EXPERIMENTAL PSYCHOLOGY

GEORGE H. ZIMNY, *Marquette University*

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THE PSYCHOLOGY OF MENTAL HEALTH

LOUIS P. THORPE, *University of Southern California*

Second Edition of this widely used textbook offers a comprehensive treatment of the main issues and topics in the field of mental health. Concentrating on the normal, reasonably well-adjusted individual, it also analyzes the causes and patterns of deviate behavior and of psychoneuroses and psychoses. Book emphasizes the influence of home, school, and community environment and the importance of prevention in maintaining mental health. *2nd Ed., 1960. 614 pp.; 145 ills., tables. \$7.00*

THE MEANING AND MEASUREMENT OF NEUROTICISM AND ANXIETY

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Just Published! This practical handbook provides a clinically meaningful and precise description of neurosis and anxiety, as derived from behavior ratings, questionnaire self-reports, and objective tests. Book coordinates data ranging from the physiological through the psychological and finally to the sociological; introduces mathematical models for more comprehensive diagnosis and accurate prognosis. *A volume in A Psychology Series edited by J. McV. Hunt. 1961. 560 pp.; 67 ills., tables. \$12*

PERSONALITY ASSESSMENT AND DIAGNOSIS

A Clinical and Experimental Technique

EDWARD BENNETT, *formerly Tufts University*

Just Published! This original study describes and applies a new technique for eliciting evidence of subjective feelings by means of multiple forced-choice judgments. The resulting quantitative multi-dimensional profile provides rich clinical material which is fully open to mathematical treatment. Book applies the technique to a number of case histories and a research project. *1961. 300 pp.; 88 ills., tables. \$8.00*

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Recent Russian Books in Psychology

Compiled by JOSEF BROŽEK
Lehigh University

This list is limited to Russian books in psychology and in neighboring fields. Next month CP will provide a list of books in psychology published in Slavic languages other than Russian and also compiled by Dr. Brožek.

History

- BABSKII, E. B. I. P. Pavlov: *Zhizn' i deyatel'nost'* (Life and work; 2nd ed.). Moscow: Gosuchpedizdat, 1959. Pp. 159.
- GRASHCHENKOV, N. I. Rol' V. M. Bekhtereva v razvitií otechestvennoi neurologii (The role of V. M. Bekhterev in the development of Russian neurology). Moscow: Medgiz, 1959. Pp. 44.
- TEREKHOV, P. G. Nikolai Evgen'evich Vvdenskii (1852-1922). Moscow: Academy of Sciences, 1958. Pp. 106.

Theory

- RUBINShteIN, S. L. *Printsipy i put razvitiya psikhologii* (Principles and paths of the development of psychology). Moscow: Academy of Sciences, 1959. Pp. 354.

Textbooks and Handbooks

- ARTEMOV, V. A. *Kurs legtsii po psikhologii* (Course of lectures in psychology; 2nd ed.). Kharkov: A. M. Gor'kii University, 1958. Pp. 421.
- IVANOV, P. I. *Psikhologiya* (Psychology; 3rd ed.). Moscow: Gosuchpedizdat, 1959. Pp. 403.
- Psikhologicheskaya nauka v S.S.S.R.* (The science of psychology in the U.S.S.R.) Vol. 1. Moscow: Academy of Pedagogical Sciences, 1959. Pp. 600.
- RUDIK, P. A. *Psikhologiya* (Psychology). Moscow: Fizkul'tura i Sport, 1958. Pp. 501.
- ZAPOROZHETS, A. V. *Psikhologiya* (Psychology). Moscow: Gosuchpedizdat, 1959. Pp. 224.

Vision

- KOROBKO, B. G. *O zrite'noi temnovoi adaptatsii* (Visual dark adaptation). Medgiz: Leningrad, 1958. Pp. 248.

Perception

- SOKOLOV, E. N. *Vospriyatie i uslovnyi refleks* (Perception and conditioned re-

flexes). Moscow: Moscow University, 1958. Pp. 333.

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Memory

- SMIRNOV, A. A. (Ed.) *Voprosy psikhologii pamyati* (Problems of the psychology of memory). Moscow: Academy of Pedagogical Sciences, 1958. Pp. 216.

Set

- PRANGISHVILI, A. S. and Z. I. KIKIAVA (Eds.) *Eksperimental'nye issledovaniya po psikhologii ustanovki* (Experimental investigations on the psychology of set). Academy of Sciences of Georgian S. S. R. 1958. Pp. 598.

Needs and Abilities

- MYASISHCHEV, V. N. (Ed.) *Psikhologiya* (Psychology). Scientific Publication No. 265. Leningrad University, 1959. Pp. 159.

Individual Differences

- TEPLOV, B. M. (Ed.) *Tipologicheskie osobennosti vysshei nervnoi deyatel'nosti cheloveka* (Typological characteristics of man's higher nervous activity). Vol. 2. Moscow: Academy of Pedagogical Sciences, 1959. Pp. 230.

Child

- KRASNOGORSKII, N. I. *Vysshaya nervnaya deyatel'nost' rebenka* (Higher nervous activity of the child). Medgiz: Leningrad, 1958. Pp. 320.
- LEVITOV, N. D. *Detskaya i pedagogicheskaya psikhologiya* (Child and educational psychology). Moscow: Gosuchpedizdat, 1958. Pp. 323.
- LURIYA, A. R. (Ed.) *Problemy vysshei nervnoi deyatel'nosti normal'nogo i anomal'nogo rebenka* (Problems of the higher nervous activity of normal and abnormal children). Vol. 2. Moscow: Academy of Pedagogical Sciences, 1958. Pp. 455.

Development

- BYKOV, K. M., A. D. SLONIM, and R. P. OL'NYANSKAYA (Eds.) *Opyt izucheniya regulyatsii fiziologicheskikh funktsii v estestvennykh usloviyakh sushchestvovaniya organizmov* (Studies on the regulation of physiological functions in natural conditions of the existence of organisms). Vol. 4. Moscow-Leningrad: Academy of Sciences, 1958. Pp. 219.
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- LEONT'EV, A. N. *Problemy razvitiya psikhiki* (Problems of mental development). Moscow: Academy of Pedagogical Sciences, 1959. Pp. 495.
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Education

- BOGOVALENSKII, D. N. (Ed.) *Voprosy psikhologii usvoeniya grammatiki i orfoografi* (Psychological problems of learning grammar and spelling). Vol. 1. Moscow: Academy of Pedagogical Sciences, 1959. Pp. 376.
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Psychotherapy

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- KERBIKOV, O. V., N. I. OZERETSKII, E. A. POPOV, and A. V. SNEZHNEVSKII. *Uchebnik psikhiiatrii* (Textbook of psychiatry). Moscow: Medgiz, 1958. Pp. 367.

LEBEDINSKII, M. S. *Ocherki psikhoterapii* (Outlines of psychotherapy). Moscow: Medgiz, 1959. Pp. 351.

SVYADOSHCH, A. M. *Nervozy i ikh lechenie* (Neuroses and their treatment). Moscow: Medgiz, 1959. Pp. 367.

Neurophysiology

BALONOV, L. YA. *Uslovnoreflektornaya regulyatsiya serdechnoi deyatel'nosti cheloveka* (Roll of conditioned reflexes in the regulation of man's cardiac function). Moscow: Leningrad, Academy of Sciences, 1959. Pp. 194.

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GILYAROVSKII, V. A., N. M. LIVENTSEV, YU. E. SEGAL', and Z. A. KIRILOVA. *Elektroson* (Electrically induced sleep: Clinical and physiological investigations). Moscow: Medgiz, 1958. Pp. 171.

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Work and Fatigue

FOL'BORT, G. V. (Ed.) *Voprosy fiziologii protsessov utomleniya i vosstanovleniya* (Physiological problems of the processes of fatigue and recovery). Kiev: Academy of Sciences of the Ukrainian S. S. R., 1958. Pp. 244.

GUR'YANOV, E. V. (Ed.) *Voprosy psikhologii truda* (Problems of the psychology of work). Moscow: Academy of Pedagogical Sciences, *Izvestiya* (Proceedings), Vol. 91, 1958. Pp. 295.

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IVANOV-SMOLENSKII, A. G., and I. V. SREL'CHUK (Eds.) *Trudy Instituta Vyshego Nervnoi Deyatel'nosti, Seriya patofiziologicheskaya* (Transactions of the Institute of Higher Nervous Activity, Pathophysiological series). Vol. 5, Experimental studies on the disturbances of higher nervous activity, especially of the interaction of the signal systems, in some neuropsychiatric diseases). Moscow: Academy of Sciences, 1958. Pp. 373.

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(Financial aid provided by the Institute of Research, Lehigh University, in obtaining the Russian publications is gratefully acknowledged by the compiler.)

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BARITZ, LOREN. *The servants of power: a history of the use of social science in American industry*. Middletown, Conn.: Wesleyan University Press, 1960. Pp. xii + 273. \$4.50.

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Modern civilization rests upon physical science, for it is physical science that makes intelligence and moral energy stronger than brute force. The whole of modern thought is steeped in science. It has made its way into the work of our best poets, and even the mere man of letters, who affects to ignore and despise science, is unconsciously impregnated with her spirit and indebted for his best products to her methods. She is teaching the world that the ultimate court of appeal is observation and experience, not authority. She is creating a firm and living faith in the existence of immutable moral and physical laws, perfect obedience to which is the highest possible aim of an intelligent being.

—THOMAS HUXLEY

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